MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. LXXX. JANUARY 9, 1920. No. 3

Professor A. FOWLER, F.R.S., PRESIDENT, in the Chair.

Jules Baillaud, Docteur-ès-Sciences, Astronome Adjoint à l'Observatoire de Paris, 148 Boulevard du Montparnasse, Paris, XIV^{e};

William Wood Burkett, 15 Vanbrugh Fields, Blackheath, S.E.;

Fred Clements, M.Inst.C.E., M.I.Mech.E., The Quintas, Spital, Chesterfield;

Ralph E. De Lury, M.A., Ph.D., Astronomer, Dominion Observatory, Ottawa, Canada;

Wilfred Hall, M.A., Prior's Terrace, Tynemouth, Northumberland;

Frank Jeffries, 120 Malpas Road, Brockley, S.E. 4;

Ernest Marsden, D.Sc., M.C., Professor of Physics, Victoria University College, Wellington, New Zealand;

Lord Rayleigh, 69 Cadogan Square, S.W. 1;

Carl Sanders, Matuba, Cabinda, Portuguese West Africa;

James Francis Schofield Shepherd, M.A., 9 Park Road, Halifax;

The Rev. Samuel Paul Shipley, The Manse, Oakham;

David Williams, B.Sc.(Lond.), Professor of Mathematics, Rhodes University College, Grahamstown, South Africa; and

Lieut.-Col. H. St. J. L. Winterbotham, C.M.G., D.S.O., R.E., Ordnance Survey, Southampton,

were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

James Garton Bower, Erlham House, Norwich (proposed by C. Thwaites);

Peter Frederick Burns, B.Sc., 64 Queen Bertha Road, Ramsgate (proposed by the Rev. J. Maurus Moorat);

Miss Marguerite Clare Dunnicliff, Mathematical Tripos, Cambridge, Eversley House, Herne Hill, London, S.E. 24 (proposed by Mrs. E. W. Maunder);

Willem Jacob Luyten, University Observatory, Leiden, Holland (proposed by Sir F. W. Dyson);

Harold William Newton, Junior Assistant, Royal Observatory, Greenwich, 128 Westcombe Hill, Blackheath, S.E. 3 (proposed by Sir F. W. Dyson); and

Ernest Tourner, The Observatory, Singapore (proposed by Lieut. W. G. Bennett).

Major P. H. Hepburn and Mr. P. J. Melotte were appointed Auditors of the Treasurer's accounts for the year 1919.

Forty-nine presents were announced as having been received since the last Meeting, including, amongst others:—

Catalogo Astrografico, Sezione Vaticana, vol. 4 (+61°), presented by the Vatican Observatory.

Seven photographs of stellar spectra taken with the 100-inch telescope, presented by the Mount Wilson Observatory.

Note (Editorial) on Andoyer's and Strömgren's Papers.

The attention of the Editors has been drawn to the fact that two papers which appeared in *Bulletin Astronomique* for 1906 February and April, by M. H. Andoyer, entitled "Sur l'Equilibre relatif de *n* Corps," covered some of the same ground as the paper by Professor Strömgren in *M.N.R.A.S.*, **80**, 12, but in an analytical form, not a numerical one. They think it well to make this reference, in order to call the attention of those interested to previous work on the subject.

Photographs taken with the 100-inch Reflector of the Mount Wilson Solar Observatory.

Professor G. E. Hale sends the following notes on the photographs recently presented to the Society:—

I am sending some preliminary results with the 100-inch telescope, which may be of interest to the Fellows of the Royal Astronomical Society.

The photographs of the moon were taken by Mr. Pease at the 134-foot Cassegrain focus on 1919 September 15.

The spectra of ε Andromedæ were made by Dr. Merrill and Dr. Strömberg in the course of a comparative test of the light-gathering power of the 60-inch and 100-inch telescopes. For the results of this test see the last number of the *Publications of the Astronomical Society of the Pacific.* There are two slips in this article that should be corrected. At the top of p. 258, the first line should read "the theoretical ratio of *light-gathering power*" instead of "the theoretical ratio of exposure times." On p. 260 I remarked, in harmony with Professor Michelson's suggestion, that an interferometer with mirrors 25 feet apart might suffice to measure the diameter of some of the largest stars. This was Professor Michelson's own idea, but Dr. Anderson, who has looked into the matter further, thinks that a much larger interferometer will be required. I have not yet had an opportunity to consider the probable maximum diameter of any star of sufficient brightness for measurement by this means. The method is one of great promise, as it is really extraordinary how sharp the interference fringes were, even when seen on a poor night with the slits nearly 100 inches apart.

I have since found that the necessary distance between the interferometer mirrors must probably range from 110 to 150 feet in order to measure the diameters of such promising stars as Capella, Sirius, Antares, and Betelgeux. We are considering the feasibility of constructing a special form of interferometer of these dimensions.

The photographs of Campbell's "hydrogen" star (B.D. + 30° 3639), which is actually a planetary nebula, was made by Mr. Pease at the 134-foot focus on 1919 August 17. He also obtained a photograph of this object with a slitless spectrograph, which is reproduced in the *Publications of the Astronomical Society of the Pacific,* but I have no slide of this. The integrated image shows very well the rift in the nebula at one side of the nucleus.

The photograph of the Ring Nebula should not be taken as a real test of the possibilities of the telescope, because when it was made the arrangements for observing at the principal focus were not completed, and two men were standing in the upper end of the tube, carried along by the driving clock while the exposure of ten minutes was being made. Naturally the star images are larger than they would be under normal conditions, but it is interesting to note how well the details of the nebula are brought out in spite of the unsatisfactory conditions under which the photograph was taken.

The slide of Messier 13 is from a negative made by Mr. Pease at the 134-foot focus on 1919 August 17. The scale of the slide is the same as the scale of the image at the 134-foot focus.

Dr. Adams sends a number of slides from recent interesting photographs of stellar spectra made with the 100-inch telescope.

Nova Aquilæ, 1919 *October* 8, *Magnitude* 7·5, *Exposure Time* 75 *minutes.*

The principal bright bands in the spectrum are the nebular bands λ 4363, N_2 and N_1. The hydrogen bands are shown faintly. All of the bands are characterised by the same peculiar structure, a strong narrow maximum greatly displaced to the violet being the principal feature.

B.D. + 23° 123, 1919 *November* 13, *Magnitude* 8·5, *Exposure Time* 226 *minutes.*

A typical R-type star showing carbon bands and a relatively strong continuous spectrum in the violet. The radial velocity of this star is − 220 km. per second.

Companion to Lalande 6888, 1919 *November* 13, *Magnitude* 8·8, *Exposure Time* 125 *minutes.*

This star has a K-type spectrum of the dwarf class. The line λ 4216 is weak and the line λ 4455 is strong. These two lines give an absolute magnitude of 7·5 and a parallax of 0″·055. The trigonometric parallax of this star is 0″·040.

Σ 2576, 1919 *November* 3, *Magnitudes* 8·2–8·2, *Exposure Time* 68 *minutes.*

This is a well-known double star, with a separation of 3″·6. The spectra of both stars are shown on the photograph. They are exactly alike in type, being about K_5 on the Harvard system. Their absolute magnitude as derived from the spectrum is 6·8, corresponding to a parallax of 0″·052.

R Aquarii, 1919 *October* 19, *Magnitude* 8·3, *Exposure Time* 120 *minutes.*

Variable star of type Md, with range from 6·2 to 11·0. This photograph was taken about one month before maximum. The nebular lines N_1, N_2, and λ 4363 are shown prominently, this being the first star in which these lines have been found, if we except Novæ. Additional photographs show that these lines become fainter near the star's maximum of light.

T Tauri, 1919 *October* 13, *Magnitude* 10, *Exposure Time* 334 *minutes.*

Irregular variable star associated with Hind's variable nebula. The spectrum shows numerous bright lines, including hydrogen, several enhanced lines, and H and K of calcium. The latter appear very rarely as bright lines in stellar spectra.

Nova Ophiuchi, 1919 *November* 2, *Magnitude* 8·8, *Exposure Time* 115 *minutes.*

This is the Nova discovered by Miss Mackie on Harvard College Observatory photographs and announced in Bulletin No. 696. It reached a magnitude of 7·5 on September 13. The spectrum is of typical Nova type in an early stage, a remarkable feature in this case, since the maximum of light apparently occurred nearly two months previously. A large number of bright bands appear accompanied by dark lines on the violet side. These bands include H and K of calcium, the hydrogen series, and numerous enhanced lines of iron and titanium. The bands are about 10 Ångströms wide, and vary in width with wave-length. The dark lines are displaced about 4·5 Ångströms to the violet at λ 4500, the displacement being directly proportional to wave-length. The corresponding displacement in Nova Aquilæ was about 23 Ångströms.

Mount Wilson Observatory:
1919 *November* 7.

On the Original Form of the Alfonsine Tables.

By J. L. E. Dreyer, M.A., Ph.D.

The Alfonsine Tables of the planets were first printed at Venice in 1483, after having been in use in manuscript form for about two hundred years. This edition was succeeded by about a dozen others, of which the best known are those of 1492 and 1518 (Venice), and the Paris edition of 1545, reprinted in 1553.* All these editions are practically identical as regards the tabular matter, and only differ as to the preface and instructions for using the tables. The positions of a planet are not given directly as in our modern tables, but the mean longitude is given for the epoch of the tables, the beginning of the Christian era (*radix incarnationis*), to which has to be added the change of mean longitude in the interval between the epoch and the time for which the position of the planet is required; and it is this quantity which is supplied by the tables. The interval elapsed since the epoch is not expressed in years and parts of a year, but in a peculiar sexagesimal system. The unit of time is a day (1^{I}), which is divided into 60 minutes, and these again into 60 seconds, and so on. On the other hand, the unit above a day is 1^{II} or 60 days, while $1^{\mathrm{III}} = 60 \times 60$ days, etc. The computer has therefore first of all to express the date in this peculiar system, which is done by a small auxiliary table, so that additional work is hereby thrown on his shoulders. But, on the other hand, the use of the sexagesimal system reduces the number of tables required for the mean

* This is the last edition, except a very belated one published at Madrid in 1641, described in Kästner's *Geschichte der Mathematik*, ii. p. 312.

longitude of each planet to one, if only we change the name of the tabular quantity each time. (In the same way we only want one table for converting hours, minutes, and seconds into arc, or *vice versa*, because $1^h = 15°$, $1^m = 15'$, and $1^s = 15''$.) This reduced the work of copying a set of tables a good deal, and in the days before the invention of printing this was an advantage worth considering. But it is more likely that the introduction of the sexagesimal system was simply made in imitation of Ptolemy, who had also divided the day into sixty minutes.

The heading of the first page of the edition of 1483 is:— "Alfontij regis castelle illustrissimi cœlestiū motuū tabule. . . . At primo Joānis saxoniēsis in tabulas alfontij canōes ordinati incipiūt faustissime." At the end of the introduction there is an account of how to calculate eclipses. "secundum doctrinam magistri Joannis de Lineriis," after which various manuscripts (which we shall describe in the following) add the words "a quo habeo scientiam meam." The author of the lengthy introduction, Johannes de Saxonia, was therefore a pupil of Johannes de Lineriis (Jean de Lignières), a well-known French astronomer who lived at Paris during the early part of the fourteenth century. This introduction is not in the edition of 1492, which contains a different one by Johannes Lucilius Santritter, though founded on the one of 1483.* This is also found in the edition of 1518.

It has therefore always been understood that Johannes de Saxonia, an otherwise unknown man, was the author of the introduction to the first printed edition of the Alfonsine Tables. But nobody had any idea that he (or his teacher) had done a good deal more to the tables than giving instructions how to use them. Very little was known about Don Alfonso's astronomical work; and most of what was supposed to be known has turned out to be erroneous. Thus it was asserted that Alfonso had assembled a congress of about fifty Arabian, Jewish, and Christian savants, from whose deliberations the Alfonsine Tables sprang. This was simply a misunderstanding, due to the fact that many of Alfonso's writings were translations (more or less free) of Arabic works; and many of the men supposed to have been present at the congress lived long before him.† Another mistake was that the tables were completed in 1252 (the year of Alfonso's accession), but that they were recomputed four years later with the simple precession of 1° in 66 years instead of the mixture of progressive and periodic precession peculiar to the tables. In reality the tables were computed nearly twenty years later, and what happened in 1256 was simply that the description of the fixed stars by Al Sufi (in which the precession of 1° in 66 years is employed) was translated into Castillian.

* It ends thus: "Expliciunt canones sive propositiones in tabulas Alfonsi clarorum Germanorum Joannis saxoniensis et Joannis lucilij Santritter heilbronnensis.

† First pointed out by the Hebrew and Arabic scholar Steinschneider in 1848.

That we are now able to form a better idea of the scientific activity of Don Alfonso is in the first instance due to the publication at Madrid in 1863–67 of the great work *Libros del Saber de astronomia del Rey Don Alfonso X. de Castilla*, edited by Manuel Rico y Sinobas (4 vols., and part i. of vol. v., in large folio). This work, never printed before, is a collection of tracts on instruments, including mechanical means of finding approximate places of the planets,* and preceded by a tract on the constellations. A great deal of the contents is taken from various Arabic writers. In addition to these tracts the work contains (in t. iv.) a treatise in fifty-four chapters, *El libro de las taulas Alfonsíes*, which is undoubtedly the original introduction to the Alfonsine Tables, never before printed, and apparently only existing in one copy, the codex L. 97 of the National Library of Madrid, which dates from the end of the fourteenth or the beginning of the fifteenth century.

In the preface to this treatise we are told that it is written by Jehuda ben Mose and Isaac ibn Sid, in the first decade of the fourth century of the second millennium after the era of Cæsar,† two hundred years having elapsed since Al Zarkali's work. As computed and observed places differed, Don Alfonso collected instruments and ordered observations to be taken at Toledo. The writers had therefore observed the sun throughout a whole year, particularly at the equinoxes and solstices and at the middle of the signs of Taurus and Scorpius, Leo and Aquarius; they also observed conjunctions of planets *inter se* and with fixed stars, and solar and lunar eclipses; they also took other observations whenever doubts arose. And when all had been examined, they adopted everything that was correct or nearly so, and from the radices [mean longitudes for some epoch] resulting from the observations they prepared the following tables and put together this book and divided it into fifty-four chapters. "And we have called it the book of the tables of Alfonso, because it was made and compiled by his orders."

We learn a good deal from this preface. First we see that there was no congress of Muhammedan and Jewish savants, but that the observations were made and the tables prepared by two Jewish astronomers. We are unfortunately unable to supplement from other sources the scanty information here given about the observations. The only astronomical writer who quotes them is Isaac Israeli of Toledo, who in his book *Jesod Olam* (written in Hebrew about the year 1310) refers to observations by Isaac ben Sid of the solar eclipse of the 5th August 1263 and of two lunar eclipses of the 24th December 1265 and the 19th June 1266.‡ The treatise on the constellations in vol. i. of the *Libros del Saber* also

* These contrivances were at the end of the Middle Ages known as *æquatoria*.

† Generally called the Spanish era, 1 Jan. 38 B.C.

‡ *Liber Jesod Olam seu Fundamentum Mundi auctore R. Isaac Israeli Hispano* (Berlin, 1848), part ii. pp. iv and xxiv.

gives 14 positions of fixed stars for the year 1260, observed at Toledo. The preface furthermore tells us that the work was done in the years 1301–1310 from the era of Cæsar, or A.D. 1263–1272, and there is no mention of any earlier redaction of the tables made in the year 1252. This year was, from the time when the tables were printed, assumed to be that in which the tables were made, solely because the "era of Alfonso" (given alongside the eras of the Deluge, Alexander, Cæsar, etc.) is June 1, 1252, or 1251 years and 152 days after the incarnation. This was the day of Alfonso's coronation. As the tables were probably not completed till the year 1272,* twenty years later than formerly supposed, it is now easier to understand the fact that they remained unknown among the savants at Paris till within a few years of the end of the thirteenth century, while the Toledo tables of Al Zarkali were still in use there.

The fifty-four chapters of the *Libro de las taulas* give full instructions as to how to use the tables, like all similar writings of those days, explaining the simplest things in what seems to a modern reader, a needlessly prolix manner. The first fourteen chapters deal with time, the various eras, Roman (*i.e.* Julian) and Muhammedan years, etc.; these do not call for any comment. But the fifteenth chapter is a surprise. It describes how to find the place of a planet for a given date. With the time you enter the table of the "annos collectos," which proceeds with intervals of 20 years; then with the remainder, less than 20, you enter the table of "annos espansos," which proceeds from year to year; after which the values for the proper months, days, hours, and minutes are taken from the respective tables. All the values found are then added together.

It is obvious that the tables, of which the use is described here, must have been very different from those afterwards printed, as there is no trace of the sexagesimal division of time; nor can the tables have given merely the *change* of longitude in a unit of time, but must have given the longitude itself for certain years. And it is not only in the way in which time is expressed that we miss the sexagesimal system; the "signs" used are the zodiacal signs of 30° (afterwards called *signa communia*) and not the signs of 60° (afterwards called *signa physica*) of the printed tables.

The following chapters deal with the planetary inequalities, and show that the printed tables of these are true copies of the original ones. This was to be expected, since the argument of these tables is the mean longitude, while the time does not enter into them. The same is the case with the table of latitudes, the description in chapter 22 agreeing perfectly with the printed tables. We may here pass over nearly the whole of the remaining chapters, many of which refer to tables which have nothing to do with planetary

* Andalo di Negro, of Genoa (first half of the fourteenth century), who wrote a number of rather worthless tracts on astronomical matters, says that the tables were finished in 1272. Duhem, who records this, is not aware of the light thrown on this question by the *Libros del Saber*, but is surprised at Andalo making such a mistake (*Le Système du Monde*, t. iv. p. 266).

motions. But we cannot pass over chapter 49, on precession ("Cuomo se a de saber enderezar el mouimiento de la cabeza de aries en longura et en tornado"). It will be remembered that according to the Alfonsine Tables precession consists of two terms, a constant one of 26″·45 a year, and a periodic one (*trepidatio*), the period of which is 7000 years. The amount of trepidation since the fundamental epoch (which here is A.D. 15, May 18·0) is $9^\circ \sin m$, where m is an angle which at that epoch was 0 and increased by 3′·086 a year. In the printed tables this angle was tabulated, and with the quantity thus found as argument the amount of trepidation ("æquatio motus accessus et recessus sphæræ stellatæ") is taken from another table. This process is described in chapter 49 of the introduction, but there is no mention of the secular term ("motus medius augium et stellarum fixarum").

We must, however, not conclude from this that the tables to which this introduction belonged ignored the existence of a constant term of precession. At the end of the year 1260 this had moved the equinox 9° 15′ since A.D. 1·0, while the periodic term could account for 8° 5′, together 17° 20′, or only 10′ less than the true amount. It was of course impossible for Don Alfonso or his two astronomers to believe that longitudes had only increased 8° 5′ since the beginning of the Christian era, or 7° 5′ since the time of Ptolemy (A.D. 140). We shall see further on that the secular term of precession was included in the mean longitudes of the *anni collecti* and in the *medius motus* of the printed tables.*

At the end of the last chapter (the 54th, on the finding of Easter) we read: "Aqui concluye el libro et se comienzan las taulas." But the codex does not contain any tables. Rico, however, publishes from two other codices some tables which he calls *Fragmentos numericos de las taulas Alfonsíes.* These have nothing to do with the tables described in the *Libro de las taulas*, but belong to the class of "perpetual almanacs" which without calculation give very rough places of the sun, moon, and planets for every day or every fifth day of certain cycles, after the lapse of which the planets return nearly to the same places in the heavens.† It is difficult to understand how Rico could imagine that this almanac had anything to do with the Alfonsine Tables. He is quite aware that the printed tables do not agree with the *Libro de las taulas*, and he seems to believe that the original tables were "falsified" at Barcelona, and that these falsified copies reached Germany and fell into the hands of John of Saxony! ‡ But there

* There is, therefore, not any discrepancy between this chapter and the well-known Alfonsine periods of 49,000 and 7000 years, as supposed by A. Wegener in an otherwise very good review of the *Libros del Saber* in the *Bibliotheca mathematica*, vi.$_3$ p. 181.

† The periods adopted are the same as those used in the perpetual almanacs of Zacuto and Santritter (Venus 8 years, Mars 79, Jupiter 83, Saturn 59). Only for Mercury the period is different, 46 years in the pseudo-Alfonsine almanac, 125 in the others. The period of 46 years is due to Al Betrugi (see my *Hist. of the Planetary Systems*, p. 266).

‡ *Libros del Saber*, t. v. p. 63.

is no foundation for this fanciful hypothesis. When we remember the slavish adoration of authorities like Aristotle and Ptolemy prevailing at that time, it is not at all remarkable that it should have seemed desirable to recast the tables of Alfonso so as to bring them nearer to the Ptolemaic model. It is also strange that Rico should have rested content with his *Fragments* and made no effort to find the original tables described in the *Libro de las taulas*. In his fifth volume he gives a descriptive catalogue of seventy-five codices of Alfonsine writings, a good many of which are supposed to contain tables. The last seventeen may be struck out at once, as two of them (62 and 63) are the tables of Al Zarkali, while the existence of the other fifteen is only assumed, their whereabouts being unknown.* From the meagre descriptions of such of the remaining codices as are said to contain tables, it is evident that some of these are of the sexagesimal kind, the *canones* (introductions) giving the name of Joh. de Saxonia; while a few of the others, preserved in continental libraries, *may* contain the tables in their original form. If so, Rico has either not seen these codices himself or has failed to understand their contents.†

Of codices in British libraries Rico enumerates six only, numbered 5 and 41 to 45, all of which I have examined. The first one‡ only deals with instruments; of the others, 41, 43, and 45§ contain nothing by or about Don Alfonso; 42‖ contains only the canon of Johannes de Saxonia and a table of mean motions in longitude for every day throughout the year. Only Rico's codex 44 (Brit. Mus., Harley 1009) contains a canon (by William Reede) and a set of tables. If Rico had seen this codex he could hardly have failed to realise that these were the original Alfonsine Tables described in the *Libro de las taulas Alfonsíes*, and very different from the printed tables.

Though Rico had quoted so very few codices from British library catalogues, it seemed to me extremely likely that many others would be found to exist in this country; while one might reasonably hope that some of these codices would turn out to contain the tables in their original form. The study of mathematics and astronomy was much cultivated at Oxford in the fourteenth century, especially at Merton College, by John Maudith, Richard Wallingford, Simon Bredon, William Reede, Walter Bryte, and others. Even though the University of Paris at a very early date adopted the modified form of the tables due to one of their own teachers, it appeared possible that Oxford might for a while have adhered to the original tables. The results of my search

* They are supposed to have been used by the editors of the printed tables.

† Rico's Codex 18, in the National Library, Madrid, of which he gives the headings in his Appendix E (t. v. p. 122), seems to be the original tables. Strange that Rico says nothing about it.

‡ Bodley, Canonici Misc. 340.

§ Brit. Museum, Harley 1, 80, and 3734. The tables in the last mentioned are those of Arzachel (Al Zarkali).

‖ Brit. Mus., Harley 531.

fully justified this hope, as the very first MS. examined contained no trace of the sexagesimal arrangement.* Though the various volumes I have seen differ a good deal as regards completeness, some omitting the tables of equations, others the tables of latitudes, others the canon, etc., I have only come across two types of tables: the sexagesimal ones, identical with the printed tables, and the original tables in the form described in the *Libro de las taulas*, but reduced to the meridian of Oxford. A list of all the codices of the original tables, which I have examined, is given at the end of this paper, as well as a copy of the most important tables. I shall now give a short description of these.†

First comes a table of the mean longitude of each planet for every twenty years, nearly always from 1340 to 1600. Only in one MS. (Bodleian Laud Misc. 594) it begins with 1320. The places are (as in all tables of those days) for the *end* of each year, and the first column is headed "anni collecti," so that this is the "taula de los annos collectos, la que crece de XX en XX" described in the Spanish introduction. The table also contains the angle in the epicycle of the moon (*argumentum lunæ*) and the same for Mercury and Venus, as the mean longitudes of these two planets are equal to those of the sun. It also gives the mean motion of the lunar node reckoned from the first point of Aries in the retrograde direction.

That these longitudes are really "super meridiem civitatis Oxon." I found by computing a number of places of the sun and moon both from the printed tables and from the MS. tables. The latter were found to correspond to a meridian 16^m east of the meridian of Toledo, and this is exactly the supposed difference of longitude between Oxford and Toledo, according to the introduction to the MS. tables.

Next follows a table of the corresponding motions "ad annos Christi expansos" from 1 to 20, the leap years being marked by a *b*. This table is succeeded by three others, one giving the value for the end of each month of the Christian (*i.e.* Julian) year, one for every day (1–30), and one for every hour (1–30). These tables all agree closely with the description in chapter 15 of the *Libro de las taulas*. The values are given in signs (of 30°, not of 60°, as in the printed tables), degrees, minutes, and seconds.

We then come to a "Tabula verarum augium ad annos Christi collectos" for the end of every twenty years from 1340 to 1600, only in two cases (so far as I have found) beginning with 1320.‡ This is a table of the apogee of the sun and planets, showing the

* I am much indebted to Dr. and Mrs. Singer of Oxford, who generously allowed me to consult their card catalogue of scientific MSS. older than the year 1500 in all the libraries of the United Kingdom.

† For the convenience of readers unacquainted with medieval astronomical expressions, I may mention that *centrum* is the arc of the excentric or deferent circle from the apogee to the centre of the epicycle; *argumentum* is the arc of the epicycle from its apogee to the planet; *radix* is the value of a quantity for some epoch; *aux* is the longitude of the apogee.

‡ Digby 48 and Laud Misc. 594.

changes caused by precession. In the printed tables the *radices incarnationis augium* are given; that is, the longitude of each apogee for the epoch of A.D. 1·0. To each *radix augis* has to be added the *aux communis* or amount of precession since the epoch, and the sum is the *aux propria* or longitude of the apogee for the date required. In the table in the codices this addition has already been carried out, so that the longitude of the apogee can be taken directly from the table. This table agrees perfectly with the Alfonsine theory of precession described above; *e.g.* for the sun and Venus we have for 1340—

Radix incarnationis . . .	2^s	11°	$25'$ $23''$
Precession, secular term . . .	9	50	42
periodic term . .	8	20	50
Aux propria ad annum completum 1340	2^s	29°	$36'$ $55''$

which agrees with the table in the codices.

This application of precession to the longitude of the apogee is the only way in which precession is ostensibly allowed for in the tables, printed and unprinted. But it is only the inequalities (*æquationes*) which depend on the *aux propria*, while the mean longitudes of course are counted from the equinox. It follows that the *Tabula mediorum motuum* must include precession, so that the equinox is that for the time being. We can now understand why there is no allusion to the secular term in chapter 49 of the *Libro de las taulas*. The writer of this chapter evidently saw that the regularity of progression of the *medius motus* in the table *ad annos expansos* required correction for the periodic term of precession, and he gave instructions for doing that. But this was apparently considered a needless refinement both in the Oxford redaction and in the printed tables, in both of which this slight irregularity is ignored. This is very excusable, since the periodic term in the fourteenth and fifteenth centuries diminished at the rate of only $9''$ or $10''$ in twenty years, which in those days was a negligible quantity. The secular term of $26''{\cdot}45$ appears in the *motus solis*, which after four years amounted to $1'\ 46''$ or $4 \times 26''{\cdot}45$, the secular term being nothing but the space passed over by the sun in $10^m\ 44^s$, by which amount the Alfonsine Tables made the tropical year shorter than the Julian year.*

The *Tabulæ æquationum* agree perfectly both with the *Libro de las taulas* and with the printed ones, except that they differ from the latter in using signs of 30°. They are often missing, which looks as if only very rough places of the planets were required. Still more frequently the latitude tables have been passed over. When they occur, they differ slightly from the description in the *Libro*, with which the printed tables agree. The latter only tabulate the latitude of the three outer planets on the assumption that the centre of the epicycle is at the point of greatest north or

* *Cf.* Reinhold, *Theoricæ nova planetarum*, Paris, 1558, f. 163^v.

south latitude on the deferent. The actual latitude is found by multiplying the tabulated value with a certain factor which is also tabulated. For Mercury and Venus the process is more complicated. In the Oxford tables this trifling calculation has already been carried out, so that the latitude can be taken out by mere inspection.

With this slight exception and that of the reduction of the first table to the meridian of Oxford, there can be no doubt that the Oxford tables represent the original Alfonsine Tables in the form in which they were issued from the Toledo Observatory about the year 1272. Only the lengthy introduction to the tables does not seem to have found its way to England, probably because it was written in Castillian and not in Latin, but partly also because it refers to many tables which had nothing to do with the motions of the planets. The English students of astronomy were also fond of making tables of all sorts, often very unnecessary ones; and not a few of the codices found in British libraries contain specimens of this misdirected energy.*

Instead of the original introduction, a shorter one was written by William Reede.† It is divided into seven paragraphs (*conclusiones*) explaining the use of the tables of mean motion and of auges, equations, oppositions of sun and moon, and the possibility of eclipses, etc. But there is nothing about latitudes. There are some trivial variations of expressions in various MSS. The preface is as follows:—‡

Incipit canon ad tabulas sequentes radicatas ad meridiem Oxon.

In nomine domini Jhesu Christi. Volentibus prognosticare futuros effectus planetarum in inferioribus istis primum secundum opus tabularum mediorum motuum oportet calculare diversitates. Sed inter tabulas nunc inuentas precisiores sunt tabule quas composuit Alfonsus Rex illustris Castelle super meridiem Tholeti. Sed istarum tabularum operatio est ita prolixa et tediosa quod vix aliqui proueniunt ad completam cognitionem planetarum. Ideo quasdam tabulas compendiosas super annos incarnationis Christi et super radices et motus tabularum Alfonsi fundatas ad leuitatem operis ordinaui et ad meridiem ciuitatis Oxonie reduxi, que distat ab occidente per 15 gradus et ab equinoctiali per 51 gr. et 50 m., cuius meridies precedit meridiem tholeti per 4 g. equinoctialis circuli, quibus correspondent 16 m vnius hore. Vnde ad doctrinam completam singulorum motuum planetarum doctrina capitulorum sequentium diligenter est notanda.

If we did not possess the original Castillian canon, we might conclude from this preface that Reede had made some very radical change in the tables. But this we know was not the case.

We have already mentioned that the lengthy introduction to the edition of 1483 was written by a certain Johannes de Saxonia,

* A particularly fine collection of miscellaneous tables is in the volume belonging to the Royal Astron. Society, which I hope to describe on another occasion.

† *Incipiunt canones super tabulas Reede.* Digby 48 and 97; Ant. a Wood D. 8 (Bodl.); Egerton 847, etc. Sometimes Reede's name is put after the heading "Tab. med. mot.," as in Laud Misc. 594, where there is no canon.

‡ From Brit. Mus., Harley 1009. Nearly the same in R.A.S. codex and in those mentioned in the preceding note.

who stated that the part dealing with eclipses was "secundum doctrinam Joannis de Lineriis." There are still a good many codices extant of this introduction or canon, and after the words just quoted there is in all of them the addition "a quo habeo scientiam meam." * This canon always begins thus (sometimes with slight alterations): "Tempus est mensura motus ut vult Aristoteles 4^to^ physicorum. . . . Sciendum igitur quod in discretione temporis ad usum tabularum Alfonsi incedimus modo philosophico per sexagenarium numerum, ut divisioni temporis sit signorum divisio correspondens." Sometimes a shorter canon is given instead of the lengthy one; it begins: "Quia ad inueniendum loca planetarum per tabulas Alfonti oportet reducere annos nobis notos." † Duhem, who is not aware that any alteration was ever made in the tables, mentions that this shorter canon exists in several codices in the *Bibliothèque Nationale*, and that it is stated to be by Joh. de Lineriis and to have been completed in the year 1322. ‡ Probably the change from the original form of the tables to the sexagesimal arrangement was due to Jean de Linières, who also wrote the short canon (*Quia ad inueniendum*), while his pupil Johannes de Saxonia only drew up the longer canon (*Tempus est mensura*) which finally was printed in 1483. §

At Paris the modified tables seem almost at once to have superseded the original ones altogether; and as it was from there that the knowledge of the tables spread to other continental countries, it is not to be wondered at that even the existence of the original tables had been forgotten before printing was invented. Apparently Paris was not the only place where the tables were modified. There is in the Bodleian a fine codex on vellum written at Padua in 1435, ‖ containing among other things *Prosdocimi de' Beldomandi Patuvini De motibus corporum supercælestium canones.* In these canons there is no trace of the sexagesimal arrangement, except that the signs used are "phisica" of 60°. Prosdocimo says that he finds the tables deduced from those of Alfonsus by Jacobus d' Dondis of Padua more convenient than the original ones. He explains that the table of mean motions begins at the beginning of our era and proceeds with intervals of 28 years as far as 1456, but that there is a gap from A.D. 112 to 1204. The tables were reduced to the meridian of Padua. As Jacopo de Dondi lived from 1298 to 1359, he was a contemporary of Johannes de Saxonia; but nobody outside Italy seems to have adopted his tables, though I have seen another set of tables with intervals of 28 years. ¶

* *E.g.* at Oxford: Digby 168, f. 135^v^; Magd. Hall ii., f. 124^v^; Brit. Mus., Egerton 889, f. 108^v^; Harley 531, f. 63^v^.

† Digby 168, f. 145; Magd. Hall ii., f. 148^v^. Brit. Mus., Sloane 407, f. 4^r^.

‡ P. Duhem, *Le système du monde*, t. iv. p. 64 (Rico's Codex 28).

§ Joh. de Saxonia seems to have lived at Paris. He wrote a commentary to the astrology of Alchabitius which was printed at Paris in 1521. According to Duhem (*l.c.*, p. 78) he brought out in 1355 *Exempla super tabulas primi mobilis et canones J. de Lineriis.*

‖ Canonici Misc., 554. There are no tables in this volume.

¶ Brit. Mus. Addit. 24071, ff. 72–73.

These begin with the year 1380 and end with 1632, and are for the meridian of Vienna. They also use physical signs of 60°. The same volume contains a treatise of astronomy, including a table of mean motions for 20 years with radix for the end of 1436 for the meridian of Vienna.

At Oxford there seems to have been a good deal of interest taken in astronomy at the beginning of the fourteenth century. There are several astronomical codices extant bearing the name of John Maudith, and dating from the year 1310; and one by Walter Evesham from 1316.* At what date the Alfonsine Tables first became known at Oxford is uncertain; but it must have been after 1320, since that year is the first one occurring in any Oxford codex; and as it has (so far) only been found in one, while all the other codices begin with 1340,† the tables were probably not much in use at Oxford till towards the middle of the century. By that time they had taken root there; and even at the beginning of the fifteenth century the original tables had not been forgotten, since there is a codex with *anni collecti* beginning with the year 1400.‡ But the sexagesimal tables probably soon after that time drove the original ones from the field.

Oxford seems to have been almost the only place where the Paris form of the tables was not at once victorious. It even penetrated to Spain. Among the codices in Spanish libraries catalogued by Rico there are several (Nos. 10, 19, 20, 23, 24) which contain the "Cánones hordenados de Juan de Saxonia" in a Castillian translation! There seem to be no tables (or only some fragments of such) belonging to these canons. But there is at Oxford a very fine codex§ containing "Tabule ad meridianum Salamantinum," which shows that the countrymen of Don Alfonso el Sabio had not forgotten him by the middle of the fourteenth century, though even they could not be content with the tables made under his direction, but had to make some little changes in them. The *anni collecti* are 1348, 1368, 1388, and so on to 1628. All quantities are given to thirds. The tables are very complete, including precession, equations, and latitude. The headings are in Latin, and so is the lengthy canon, which begins by saying that the tables of Alfonso are too difficult and the *modus operandi* too prolix for the students of astronomy at Salamanca, for which reason "alie tabule utilissime que vocantur resolute" have been prepared. This is perhaps the first time that the name of *Tabulæ resolutæ* was given to a set of planetary tables. This name is also found in

* Bodleian, Laud Misc. 674, f. 67–72: Tabulæ augmenti longissimi diei supra diem equinoctii pro omni terra habitabili (marked on f. 69 Tabule Mawdith facte in Oxon. 1310), Tabula ascensionis signorum . . . verificata Oxon. A.D. 1310. And f. 75 76, Declaratio motus octaue spere secundum M. Walterum Evesham Anno X^{i} 1316.

† Except a table with "radix Oxonie, 1327," *ibid.*, f. 46^{r}.

‡ Bodleian, Ant. a Wood D. 8, ff. 67–85. Some of the tables are very carelessly copied.

§ Canonici Misc. 27, ff. 33^{r}–147^{r}.

a fifteenth-century MS. of tables for the meridian of Prague;* the *anni collecti* are 1428, 1448 . . . 1808, and the tables may have been adapted from the Salamanca tables. After the heading there has been added in a different hand: "Que appellantur Tabule Auree et resolute Alberti bru. . ." (edge of paper cut). At the end of the tables is the addition: "Expliciunt Tabulæ aureæ et resolutæ Albert Bruzæ." These tables are therefore by Albertus de Brudzevo, the teacher of Copernicus at the University of Cracow.

These Salamanca tables were at last printed at Nürnberg in 1536 by Johannes Schoner under the title: "Tabulæ astronomicæ qvas uulgo, quia omni difficultate & obscuritate carent, Resolvtas uocant." The *anni collecti* go from 1388 to 2008. Neither in Melanchthon's preface nor in the editor's introduction is there any mention of Alfonso nor of any previous *Tabulæ resolutæ*. Although the Alfonsine Tables had been superseded by the Prutenic Tables in 1553, a new edition of Schoner's *Tabulæ resolutæ* came out at Wittenberg in 1588.

Why the *anni collecti* in these tables are not multiples of 20, but start from 1348, is not explained anywhere. But most probably this arrangement dates from Oxford, and is due to William Reede, since several sets of tables for the meridian of Oxford start with radices for 1348 and motion in *anni expansi* up to 10,000!†

The tables which follow contain the mean motions in longitude and those of the apogees, restored to the form in which they originally were included in the Alfonsine Tables. The only difference is, that they have been (in the fourteenth century) reduced to the meridian of Oxford, which was supposed to be 16^m east of Toledo. Together with the tables of inequalities (*tabulæ æquationum*) and latitudes in the old printed editions, they represent all those parts of the original tables necessary for calculating the place of a planet at any time within the period when the tables were in use. These tables have here been copied from the Roy. Astr. Soc. Codex, but have been collated with Digby 48 and Laud Misc. 594, and a few trivial corrections made. The *motus medii* for 1320 are from Laud Misc. 594. To save space, extracts only of the fourth and fifth tables are printed here.

* Canonici Misc. 499, ff. 72^r–118^v. There is also in this volume a set of sexagesimal tables with the canon *Tempus est mensura* (ff. 1–70).

† Laud Misc. 594, f. 70; Digby 57, f. 97^v; R. Astr. Soc. Codex, ff. 36^v and 37^r. 1348 was the year in which the Black Death came to England. Is that perhaps the reason why it was chosen as an epoch, like the Deluge formerly?

Tabula radicum mediorum motuum omnium planetarum ad annos Christi collectos super meridiem civitatis Oxon.

Anni collecti.	Motus solis.	Motus lunæ.	Argumentum lunæ.	Motus capit. Drac.	Motus Saturni.	Motus Jouis.	Motus Martis.	Argumentum Veneris.	Argumentum Mercurii.
	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″
1320	9 18 2 14	9 27 48 22	10 1 44 30	2 2 46 10	0 23 52 8	9 28 41 12	11 20 15 46	0 10 50 40	9 9 46 34
1340	9 18 11 3	2 11 22 10	11 11 28 57	2 29 36 1	8 28 33 45	6 5 55 49	7 8 35 44	6 13 44 31	9 24 14 13
1360	9 18 19 52	6 24 55 57	0 21 13 23	3 26 25 53	5 3 15 22	2 13 10 25	2 26 54 42	0 17 23 22	10 8 41 52
1380	9 18 28 41	11 8 29 45	2 0 57 49	4 23 15 44	1 7 56 59	10 20 25 1	10 15 13 39	6 21 2 13	10 23 9 31
1400	9 18 37 30	3 22 3 33	3 10 42 16	5 20 5 35	9 12 38 37	6 27 39 38	6 3 32 37	0 24 41 4	11 7 37 10
1420	9 18 46 19	8 5 37 20	4 20 26 42	6 16 55 26	5 17 20 14	3 4 54 14	1 21 51 35	6 28 19 55	11 22 4 49
1440	9 18 55 8	0 19 11 8	6 0 11 8	7 13 45 17	1 22 1 51	11 12 8 50	9 10 10 33	1 1 58 45	0 6 32 27
1460	9 19 3 57	5 2 44 55	7 9 55 35	8 10 35 9	9 26 43 28	7 19 23 26	4 28 29 31	7 5 37 36	0 21 0 6
1480	9 19 11 46	9 16 18 43	8 19 40 1	9 7 25 0	6 1 25 5	3 26 38 3	0 16 48 29	1 9 16 27	1 5 27 45
1500	9 19 21 35	1 29 52 30	9 29 24 27	10 4 14 51	2 6 6 42	0 3 52 39	8 5 7 27	7 12 55 18	1 19 55 23
1520	9 19 30 24	6 13 26 18	11 9 8 54	11 1 4 42	10 10 48 19	8 11 7 15	3 23 26 25	1 16 34 9	2 4 23 3
1540	9 19 39 13	10 27 0 6	0 18 53 20	11 27 54 33	6 15 29 56	4 18 21 52	11 11 45 22	7 20 13 0	2 18 50 42
1560	9 19 48 1	3 10 33 53	1 28 37 46	10 24 44 25	2 20 11 33	0 25 36 28	7 0 4 20	1 23 51 51	3 3 18 21
1580	9 19 56 50	7 24 7 41	3 8 22 13	1 21 34 16	10 24 53 11	9 2 51 4	2 18 23 18	7 27 30 42	3 17 45 59
1600	9 20 5 39	0 7 41 28	4 18 6 39	2 18 24 7	6 29 34 48	5 10 5 40	10 6 42 16	2 1 9 33	4 2 23 38

Tabula mediorum motuum planetarum ad annos Christi expansos.

Anni expansi.	Motus solis.	Motus lunæ.	Argumentum lunæ.	Motus capit. Drac.	Motus Saturni.	Motus Jouis.	Motus Martis.	Argumentum Veneris.	Argumentum Mercuri.
	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″
1	11 29 45 39	4 9 23 3	2 28 43 15	0 19 19 42	0 12 13 35	1 0 20 29	6 11 17 5	7 15 1 42	1 23 56 47
2	11 29 31 19	8 18 46 5	5 27 26 30	1 8 39 24	0 24 27 9	2 0 40 58	0 22 34 10	3 0 3 23	3 17 53 34
3	11 29 16 58	0 28 9 8	8 26 9 44	1 27 59 6	1 6 40 44	3 1 1 27	7 3 51 16	10 15 5 5	5 11 50 21
b 4	0 0 1 46	5 20 42 46	0 7 56 53	2 17 21 58	1 18 56 19	4 1 26 55	1 15 39 48	6 0 43 46	7 8 53 32
5	11 29 47 25	10 0 5 48	3 6 40 8	3 6 41 40	2 1 9 54	5 1 47 44	7 26 56 53	1 15 45 28	9 2 50 19
6	11 29 33 5	2 9 28 51	6 5 23 23	3 26 1 22	2 13 23 29	6 2 7 53	2 8 13 48	9 0 47 10	10 26 47 6
7	11 29 18 44	6 18 51 53	9 4 6 38	4 15 21 4	2 25 37 4	7 2 28 22	8 19 31 3	4 15 48 51	0 20 43 53
b 8	0 0 3 32	11 11 25 31	0 15 53 47	5 4 43 56	3 7 52 39	8 2 53 51	3 1 19 35	0 1 27 32	2 17 47 4
9	11 29 44 11	4 20 48 34	3 14 37 1	5 24 3 38	3 20 6 14	9 3 14 20	9 12 36 40	7 16 29 18	4 11 43 50
10	11 29 34 50	8 0 11 36	6 23 20 16	6 13 23 20	4 2 19 48	10 3 34 49	3 23 53 46	3 1 30 55	6 5 40 37
11	11 29 20 30	0 9 34 39	9 12 3 31	7 2 43 2	4 14 33 23	11 3 55 18	10 5 10 51	10 16 32 37	7 29 37 34
b 12	0 0 5 17	5 2 8 17	0 23 50 40	7 22 5 55	4 26 48 58	0 4 20 46	4 16 59 23	6 2 11 19	9 26 40 35
13	11 29 40 57	9 11 31 19	3 22 33 44	8 11 25 37	5 9 2 33	1 4 41 15	10 28 16 28	1 17 13 0	11 20 37 22
14	11 29 36 36	1 20 54 22	6 21 17 9	9 0 45 19	5 21 16 8	2 5 1 44	5 9 33 33	9 2 14 42	1 14 34 9
15	11 29 22 25	6 ♂ 17 24	9 20 0 24	9 20 5 0	6 3 29 42	3 5 22 13	11 20 40 38	4 17 15 24	3 8 30 56
b 16	0 0 7 3	10 22 51 2	1 1 47 33	10 9 27 53	6 15 45 18	4 5 47 41	6 2 39 10	0 2 55 5	5 5 34 7
17	11 29 52 43	3 2 14 5	4 0 30 48	10 28 47 35	6 27 58 52	5 6 8 10	0 13 46 15	7 17 58 46	6 29 30 54
18	11 29 38 22	7 11 37 7	6 29 14 3	11 18 7 17	7 10 12 27	6 6 28 39	6 25 13 21	3 2 58 28	8 23 27 41
19	11 29 24 1	11 21 0 10	9 27 57 18	0 7 26 59	7 22 26 2	7 6 49 8	1 6 30 26	10 18 0 10	10 17 24 28
b 20	0 0 8 49	4 13 33 48	1 9 44 26	0 26 42 52	8 4 41 37	8 7 14 36	7 18 18 58	6 3 38 51	0 14 27 39

Tabula mediorum motuum planetarum ad menses.

	Motus solis.	Motus lunæ.	Argumentum lunæ.	Motus capit. Drac.	Motus Saturni.	Motus Jouis.	Motus Martis.	Argumentum Veneris.	Argumentum Mercurii.
	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″
Jan.	1 0 33 18	1 18 28 6	1 15 0 53	0 1 38 30	0 1 2 19	0 2 34 37	0 16 14 46	0 19 6 43	3 6 18 28
Feb.	1 28 9 11	1 27 24 26	1 20 50 4	0 3 7 27	0 1 58 35	0 4 54 16	1 0 55 12	1 6 22 28	6 3 17 44
Mar.	2 28 42 29	3 25 52 32	3 5 50 46	0 4 45 57	0 3 0 53	0 7 28 53	1 17 9 58	1 25 29 11	9 9 36 12
Apr.	3 28 16 39	4 21 10 3	4 7 47 55	0 6 21 16	0 4 1 11	0 9 53 32	2 2 53 17	2 13 58 55	0 12 48 15
Mai.	4 28 49 57	6 9 38 8	5 22 48 48	0 7 59 46	0 5 3 29	0 12 33 8	2 19 8 3	3 3 5 38	3 19 6 43
Jun.	5 28 24 7	7 14 55 39	6 24 45 54	0 9 35 5	0 6 3 45	0 15 2 46	3 4 51 23	3 21 35 22	6 22 18 47
Jul.	6 28 57 25	9 3 23 44	8 9 46 39	0 11 13 35	0 7 6 5	0 17 37 23	3 21 6 9	4 10 42 5	9 28 37 11
Aug.	7 29 30 43	10 21 51 50	9 24 47 32	0 12 52 4	0 8 8 23	0 20 12 0	4 7 20 55	4 29 44 44	1 4 55 43
Sept.	8 29 4 53	11 27 9 21	10 20 44 31	0 14 27 23	0 9 8 41	0 22 41 37	4 23 4 14	5 18 18 32	4 8 7 47
Oct.	9 29 38 11	1 15 36 26	0 11 45 23	0 16 5 53	0 10 10 59	0 25 16 14	5 9 19 0	6 7 25 15	7 14 26 15
Nov.	10 29 12 21	2 20 54 57	1 13 42 22	0 17 41 12	0 11 11 16	0 27 45 52	5 25 2 19	6 25 34 59	10 17 38 19
Dec.	11 29 45 39	4 9 23 3	2 28 43 15	0 19 19 42	0 12 13 35	1 0 20 29	6 11 17 5	7 15 1 42	1 23 56 47

Tabula mediorum motuum planetarum ad dies mensium.

Dies.	Motus solis.	Motus lunæ.	Argumentum lunæ.	Motus capit. Drac.	Motus Saturni.	Motus Jouis.	Motus Martis.	Argumentum Veneris.	Argumentum Mercurii.
	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″	s ° ′ ″
1	0 0 59 8	0 13 10 35	0 13 3 54	0 0 3 11	0 0 2 1	0 0 4 59	0 0 31 27	0 0 36 59	0 3 6 24
2	0 1 58 17	0 26 21 10	0 26 7 48	0 0 6 21	0 0 4 1	0 0 9 59	0 1 2 53	0 1 13 59	0 6 12 48
3	0 2 57 24	1 9 31 45	1 9 11 42	0 0 9 32	0 0 6 2	0 0 14 58	0 1 34 20	0 1 50 58	0 9 19 22
4	0 3 56 33	1 22 42 20	1 22 15 36	0 0 12 43	0 0 8 2	0 0 19 57	0 2 5 47	0 2 27 58	0 12 25 37
5	0 4 55 42	2 5 52 55	2 5 19 30	0 0 15 53	0 0 10 3	0 0 24 56	0 2 37 13	0 3 4 57	0 15 32 1
6	0 5 54 50	2 19 3 30	2 18 23 24	0 0 19 4	0 0 12 4	0 0 29 56	0 3 8 40	0 3 41 57	0 18 38 25
7	0 6 53 58	3 2 14 5	3 1 27 18	0 0 22 15	0 0 14 4	0 0 34 55	0 3 40 7	0 4 18 56	0 21 44 49
8	0 7 53 7	3 15 24 40	3 14 31 12	0 0 25 25	0 0 16 5	0 0 39 54	0 4 11 33	0 4 55 56	0 24 51 13
9	0 8 52 15	3 28 35 15	3 27 35 6	0 0 28 36	0 0 18 5	0 0 44 53	0 4 43 0	0 5 32 55	0 27 57 37
10	0 9 51 23	4 11 45 50	4 10 39 0	0 0 31 46	0 0 20 6	0 0 49 53	0 5 14 26	0 6 9 55	1 1 4 1
20	0 19 42 47	8 23 31 40	8 21 17 59	0 1 3 32	0 0 40 12	0 1 39 45	0 10 28 53	0 12 19 49	2 2 8 3
30	0 29 34 10	1 5 17 31	1 1 56 59	0 1 35 19	0 1 0 18	0 2 29 38	0 15 43 19	0 18 29 44	3 3 12 4

Tabula mediorum motuum planetarum ad horas.

Horæ.	Motus solis.	Motus lunæ.	Argumentum lunæ.	Motus capit. Drac.	Motus Saturni.	Motus Jouis.	Motus Martis.	Argumentum Veneris.	Argumentum Mercurii.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ′	° ′ ″
1	0 2 28	0 32 56	0 32 40	0 0 8	0 0 5	0 0 12	0 1 19	0 1 32	0 7 46
2	0 4 56	0 5 53	1 5 20	0 0 16	0 10	0 25	1 37	3 5	0 15 32
3	0 7 24	1 38 49	1 37 59	0 0 24	0 15	0 37	3 56	4 37	0 23 18
4	0 9 51	2 11 46	2 10 39	0 0 32	0 20	0 50	5 14	6 10	0 31 4
5	0 12 19	2 44 42	2 43 19	0 0 40	0 25	1 2	6 33	7 42	0 38 50
6	0 14 47	3 17 39	3 15 58	0 0 48	0 30	1 15	7 52	9 15	0 46 36
7	0 17 15	3 50 35	3 48 38	0 0 56	0 35	1 27	9 10	10 47	0 54 22
8	0 19 43	4 23 32	4 21 18	0 1 4	0 40	1 40	10 29	12 20	1 2 8
9	0 22 11	4 56 28	4 53 58	0 1 12	0 45	1 52	11 48	13 52	1 9 54
10	0 24 38	5 29 25	5 26 37	0 1 19	0 50	2 5	13 6	15 25	1 17 40
20	0 49 17	10 58 49	10 53 14	0 2 39	1 40	4 9	26 12	30 50	2 35 20

Tabula verarum augium ad annos Christi collectos.

	Aux Solis et Veneris.				Aux Saturni.				Aux Jouis.				Aux Martis.				Aux Mercurii.				Diff.		Unius Anni.
	S	°	′	″	S	°	′	′	S	°	′	″	S	°	′	″	S	°	′	″	′	″	″
1320	2	29	24	23	8	11	22	42	5	21	36	0	4	13	11	13	6	28	38	33	12	31	38
1340	2	29	36	54	8	11	35	13	5	21	48	31	4	13	23	44	6	28	51	4	12	22	37
1360	2	29	49	16	8	11	47	35	5	22	0	53	4	13	36	6	6	29	3	26	12	12	37
1380	3	0	1	28	8	11	59	47	5	22	13	5	4	13	48	18	6	29	15	38	12	5	36
1400	3	0	13	33	8	12	11	52	5	22	25	10	4	14	0	23	6	29	27	43	11	52	36
1420	3	0	25	25	8	12	23	44	5	22	37	2	4	14	12	15	6	29	39	35	11	43	35
1440	3	0	37	8	8	12	35	27	5	22	48	45	4	14	23	58	6	29	51	18	11	31	35
1460	3	0	48	39	8	12	46	58	5	23	0	16	4	14	35	29	7	0	2	49	11	23	34
1480	3	1	0	2	8	12	58	21	5	23	11	39	4	14	46	52	7	0	14	12	11	12	34
1500	3	1	11	14	8	13	9	33	5	23	22	51	4	14	58	4	7	0	25	24	10	58	33
1520	3	1	22	12	8	13	20	31	5	23	33	49	4	15	9	2	7	0	36	22	10	52	33
1540	3	1	33	4	8	13	31	23	5	23	44	41	4	15	19	54	7	0	47	14	10	44	32
1560	3	1	43	48	8	13	42	7	5	23	55	25	4	15	30	38	7	0	57	58	10	34	32
1580	3	1	54	22	8	13	52	41	5	24	5	59	4	15	41	12	7	1	8	32	10	23	31
1600	3	2	4	45	8	14	3	4	5	24	16	22	4	15	51	35	7	1	18	55			

The following is a list of the codices examined, which were found to contain copies of the original Alfonsine Tables or parts of such. The list is doubtless not complete even as regards British libraries, and in particular it must be noted that I have not had an opportunity of examining more than two Cambridge MSS.* But I feel sure that nothing would be gained by extending the investigation further.

The Royal Astronomical Society Codex is contained in the first eight leaves of a volume of tables, all prepared at Oxford in the fourteenth century. A short account of this volume was given by Mr. Harris, Assistant Secretary, in vol. xv. of the *Memoirs*. It is there stated (p. 180) that "the tables bear the general title Almanac editum Oxonie." But there is no general title of any kind, and the words quoted are only written in tiny letters at the extreme top of the first page. Mr. Harris says that the "Almanac appears to have been formed from the famous Alfonsine Tables . . . and some trifling alterations made in the method of arrangement to suit the convenience of computers." It has been shown in the present paper that the differences between this MS. and the printed tables are anything but trifling. The longitudes in the tables are for the end of the year, not for January 1, as stated in the account.

* Gg. 6·3 and Hh. 6·8, but the tables in both these codices turned out to be those of Al Zarkali.

List of Codices examined

and found to contain Alfonsine Tables in their original form. Except where otherwise stated, the mean longitudes are for the meridian of Oxford.

I. *Bodleian Library.*

Laud Misc. 594. ff. 41–45, mean long. 1320–1600, motion in single years (1–20), months and days (not finished). No tables of equations nor canon. ff. 70–71, radix 1348 and anni expansi up to 10,000. Two sets of latitude tables, ff. 74–78 and 82–83.

Laud Misc. 674. One leaf (45) probably inserted later; mean long. 1340–1600 and single years. f. 46^{r}, radix Oxonie 1327 with motion in 40, 60 . . . 10,000 years. 46^{v}–60^{v}, anni expansi 1–20 with months, etc., table of aux Solis et Veneris 1400–2000; tables of equations.

Digby 17. f. 15^{v}, mean long. 1340–1600.

Digby 48. ff. 157–176, mean long. 1340–1600, with years 1–20, months, etc.; tables of equations, two tables of motus augium; the second one begins 1320. ff. 177–181, Reede's canon.

Digby 57. ff. 32–36, mean long. 1340–1600. ff. 40–43, Reede's canon, beginning missing. ff. 97^{v}–99^{v}, tables with radix 1348.

Digby 97. ff. 14–32, mean long. 1340–1600, anni expansi, table of auges, equations; latitude tables like printed ones. ff. 33–39, elaborate latitude tables (same as in Laud 594, f. 74, and R.A.S. codex, ff. 29–35). ff. 64–71, Reede's canon.

Ant. a Wood D. 8. f. 48^{v}, Reede's canon, but only the beginning. ff. 67–85, mean long. 1400–1600 (badly copied), years, months etc.; table of auges and equations.

Canonici Misc. 27. ff. 33–110, mean long. for meridian of Salamanca for 1348, 1368, 1388 . . . 1628, with anni expansi, months, etc., tables of auges, equations, latitudes. ff. 122–147, detailed canon.

Canonici Misc. 499. After a complete set of sexagesimal tables, ff. 72–106 contain tables for the meridian of Prague, mean long. for 1428, 1448 . . . 1808, precession tables, and tables of equations. ff. 126–133, latitude tables. ff. 122–125, canon.*

Canonici Misc. 554. ff. 73^{v}–93^{v}, canon by Prosdocimo de Beldomandi to Jacopo de Dondi's extract from the Alfonsine Tables for the meridian of Padua. No tables. *Cf.* above, p. 252.

II. *Oxford College Libraries.*

Magd. Hall i. (Hertford College 3). ff. 40^{v}–43^{r}, Radix Christi with motions in years 1–20, 40 . . . 2000.

Corpus Christi 144. ff. 106–109, motion in years 1–20, 40 . . . 4000, hours and minutes. ff. 110–113, tables of equations.

III. *Royal Astronomical Society.*

R.A.S. Codex. ff. 1^{r}–8^{v}, mean long. 1340–1600, anni expansi 1–20, mean motion in months, days, hours, table of auges, tables of conjunctions and oppositions of sun and moon, tables of equations; Reede's canon. ff. 29–35, latitude tables. 36^{v}–37^{r}, radix 1348 and mean motions in 1–20 . . . 10,000 years.

IV. *British Museum.*

Sloane 407. ff. 39–56, mean long. 1340–1600, with single years, months, etc. Tables of auges and equations. No canon.

Egerton 847. ff. 122–142, Reede's canon, mean long. 1340–1600, with years, months, etc., auges and equations.

---

* Begins: "Incipiunt Canones tabularum Cruciferi Gaerparum per eundem de tabulis Alfoncij ad meridianum Wratislaviensem, hic autem sunt reductæ ad meridianum pragensem." Ends: "Expliciunt tabule Cruciferi cum suis canonibus de medijs et veris motibus planetarum reducte ad meridianum pragensem. Scriptæ Anno Domini 1471 per Johannem B. de Bazyn in collegio beatissime Marie virginis nationis Boemorum domus Beczkonis."

Egerton 889. ff. 1–4, mean motion in 1–20 . . . 8000 years, also in months. Unfinished solar and lunar tables.
Harley 1009. ff. 3–11^{r}, Reede's canon, mean long. 1340–1600, with years, months, etc.
Addit. 24071. f. 5, radix for meridian of Vienna for end of 1436, with mean motion in 1–20 years. ff. 72–73, mean long. for 1380, 1408, 1436 . . . 1632. Motion in annis expansis 1–28, months, days, hours.

V. *Royal Observatory, Edinburgh.*

Crawford 28. Tabule mediorum motuum . . . in annis collectis ; first radix [which is equal to the radix incarnationis of the Alfonsine Tables reduced to a meridian $49^{m}\ 20^{s}$ east of Toledo, probably Paris], then motion in 20, 40 . . . 100 and up to 3000 years, also in months, days, and hours.

Oxford:
1920 *Jan.*

The Displacement of Spectrum Lines and the Equivalence Hypothesis. By Prof. W. G. Duffield, D.Sc.

While it is extremely doubtful whether it would be possible to conduct more careful sets of experiments than those of St. John* and of Evershed,† which were undertaken to test the displacement of solar spectrum lines with regard to the predicted relativity effect, it still remains to review the causes of relative displacements of lines of solar and terrestrial origin, and to discuss whether it is possible to obtain conclusive evidence which will either definitely confirm or refute the equivalence hypothesis.

It is well known that the magnitude of the displacement predicted by Einstein is approximately 0·008 Å.U. towards the red end of the spectrum in the neighbourhood of $\lambda = 4{,}000$ Å.U. In the following discussion are considered all causes known to the writer, which require to be taken into account in a rigorous inquiry into the existence of a gravitational displacement.

The displacement of spectrum lines may be due to the following influences, apart from instrumental defects:—

(*a*) *Pressure, and perhaps Density.*—These two influences have not been completely separated. The rate of displacement varies nearly as the third power of the wave-length.‡ Different types of lines are displaced by different amounts. In the iron arc, for example, Groups I., II., and III. are displaced ·0015, ·0033, and ·0068 Å.U. per atmosphere respectively.

(*b*) *Line-of-sight Motion.*—The rate of displacement varies as the first power of the wave-length.

(*c*) *Relativity Effect.*—The rate of displacement varies as the first power of the wave-length, and is in this respect indistinguishable from the effect of line-of-sight motion.

(*d*) *The Pressure or Density Gradient.*

* St. John, *Astrophysical Journal*, **46**, 249, 1917.
† Evershed, *Observatory*, **41**, 531, Oct. 1918.
‡ Duffield, *Phil. Trans.*, A, **209**, p. 205, 1908; **A**, **211**, p. 33, 1910.

(*e*) *Anomalous Dispersion* depending upon (*d*) and upon the position of the observer relative to the normal to the surface from which light is emitted.

(*f*) *Intensity Effect.*—The writer obtained some evidence that intense lines in arc spectra are more displaced under pressure than weak ones.* There is also some indication of a similar effect in the solar spectrum.

(*g*) *Influence of the Surrounding Atmosphere of Vibrating Atoms.*—The writer obtained some evidence that the spectrum lines emitted by an atom of a metal surrounded by foreign atoms are not displaced as much under pressure as when surrounded by similar atoms.

(*h*) *Influence of Neighbouring Spectrum Lines upon one Another.*—This effect includes both a purely photographic effect and one which, it has been suggested, has its origin in a change in the atomic frequency of vibration. We may also include the question of "blends," or close lines not perfectly resolved.

If we are comparing solar displacements with those in an arc we have further to consider—

Arc Conditions:—

(*i*) The length of arc employed.

(*j*) The current in the arc.

(*k*) *Regional or Pole Effect.*—The portion of the arc which is focussed upon the slit of the spectroscope.

(*l*) *Zeeman and Stark Effects.*—Magnetic and electric fields may impose a dissymmetry upon the lines which imitates a displacement of their centre of gravity.

(*m*) It is probable too that the nature of the absorption in the sun must be taken into account, whether due to temperature, scattering, or to ionic bombardment. There is some suspicion that the velocity or intensity of electronic impacts influences the wave-length of the emitted radiation, and there may be a difference between absorption due to resonance and that due to ionisation of the molecules.

(*n*) The width of a line may be such as to prevent its centre of gravity from being discovered.

While all the above are not independent of one another, they constitute influences which should be separately considered.

It should be clear that the properties stated in (*a*) should enable the pressure or density displacement to be separated from the effects of (*b*) and (*c*), provided that the other influences are inoperative. That they are operative in lines affected by pressure is shown by the lack of uniformity in the results of all attempts to separate out the pressure influence from the other influences in the sun.

To give an example: Assuming that only (*a*), (*b*), and (*c*) need be taken into account, we may write the displacement $\delta\lambda$ in the following form:—

$$\delta\lambda = \alpha\lambda + \beta\lambda^n\gamma p + \delta,$$

* Duffield, *Phil. Trans.*, A, 215, p. 205, 1915.

where α is a constant depending upon (1) the relativity displacement and (2) line-of-sight motion.

β ,, ,, ,, the nature of the glowing vapour.

γ ,, ,, ,, the pressure-group to which the line belongs. = 1, 2, or 4·5 approx. for the writer's Groups I., II., and III. for iron.*

δ ,, ,, ,, any cause, such as a constant error in the tables, which affects all lines equally.

n ,, a number not very different from 3.

It is obviously necessary to make observations over a wide range of wave-lengths in order to separate α from δ, but from a knowledge of the constants β and γ for different groups we should be able to obtain the value of the pressure p from a short range of the spectrum. Employing the lines used by Evershed (*Kodaikánal Observatory Bulletin*, No. 18, 1909), but departing from his method of procedure because it is open to the objection that he has combined my Groups II. and III. into one group, and solving in accordance with the above equation, I find by taking the groups in pairs the following inconsistent values for the *absolute* pressure in the sun's reversing layer: 2·8 atms., *minus* 33 atms. (an impossible value), and 1·7 atms. I have used the mean displacement per atmosphere, *i.e.* $\beta\gamma$, given in my early paper, but equally discordant results would be obtained by any reasonable estimate of the pressure displacements.

In view of the fact that we are liable to arrive at absurd values for the solar pressure, even when we use lines whose pressure displacements have been the subject of exhaustive examination, when we omit the disturbing influences of (d) to (n) inclusive, we have ample warning that some of them involve factors of very great importance, and that they must not be neglected.

The unfortunate thing is that we know so little about some of these effects that we are unable to evaluate them. I am inclined to regard (d), the displacement depending upon the gradient, as of very great importance; elsewhere † it has been shown that a solar line must be built up of a number of intensity curves, each corresponding to a different layer in the sun, and that the shape and amplitude of each component curve depend upon the density, temperature, scattering power, and pressure in the particular layer. The observed solar line is the intensity curve corresponding to the sum of all these component curves, and is hence not produced under one uniform pressure or any other uniform physical condition. It is clear that any adventitious

* Duffield, *Phil. Trans.*, A, **208**, p. 111, 1908.

† Duffield, *Qtly. Jnl. of Roy. Met. Soc.*, **41**, No. 175, p. 177, 1915; *Phil. Mag.*, **30**, 385, 1915. See also Abbot, Hahn, and St. John.

absorption in one particular layer, such as might be due to ionic bombardment, or other cause of ionisation, might appreciably influence the position of the centre of gravity of the summation line and cause a change in its apparent displacement.

Part of the discordance discussed in the previous paragraph is quite possibly due to the difference in the type of pressure or density gradients in the sun and arc. That the solar and arc gradients are different is apparent from the fact that one gives an absorption spectrum and the other a bright line spectrum, and also from the fact that for a given element the displacement of lines which are the most sensitive to pressure in the arc (unsymmetrical lines) are often the least displaced in the sun,* and *vice versa*. It has been shown that under pressure the reversed part of a spectrum line in the arc is often displaced less than that of the unreversed bright portion,† and for this reason, amongst others, the Kodaikánal‡ and Mt. Wilson§ observers regard unsymmetrical lines with great suspicion and as unreliable for the determination of physical quantities in the sun.

The wave-lengths emitted by different parts of an arc, (*k*), are in many cases very discordant—the displacement being greatest near the negative pole, where lines are displaced in the direction of their widening; here the displacement amounted to 0·1 Å.U. for 18 lines.‖ For the sodium pair 6154, 6160 a displacement at the pole of 0·52 Å.U. was observed. According to Royds, the displacements at the positive were about half those at the negative pole. If only the positive pole was supplied with material fed into a carbon arc the displacements at the negative pole were reduced. This effect may possibly be classed with those due to gradients in the arc (*d*), or to the Stark effect (*l*), but it is probable that it is related to the intensity of the electronic bombardment to which the atoms are subjected, which will be different in different regions of the arc. The existence of an intense stream of electrons near the cathode has recently been mechanically demonstrated,¶ and this appears to have its velocity increased by a further potential drop at a very short distance from the pole face. If the radiation is influenced by the intensity of the bombardment, we should expect to find a large effect near the cathode. Similar effects may exist in the sun, where there is ample cause for photo-electric and thermionic emission of electrons.

The large differences observed by Royds in the wave-lengths of unsymmetrical lines emitted from long and short arcs (*i*) are due either to consequent changes in the gradients, or to the effects considered under (*m*). To give numerical examples, Royds found that in passing from a 2-mm. to a 7-mm. arc, symmetrical lines

* Evershed, *Kodaikánal Observatory Bulletin.*

† Duffield, *loc. cit.*

‡ Royds, *Kodaikánal Observatory Bulletin*, No. 38.

§ St. John, *Ap. J.*, **44**, 311, 1916.

‖ Royds, *Kodaikánal Observatory Bulletin*, No. 40; and St. John and Babcock, *Ap. J.*, **42**, 231, 1915.

¶ Duffield, Burnham and Davis, *Phil. Trans. Roy. Soc.*, No. 574, 1919.

did not show a change in λ greater than ·0006 Å.U., whereas lines unsymmetrically broadened suffered a change of ·0085 Å.U.

That under certain circumstances the strength of the current in the arc (*j*) affects the wave-length is likewise, I think, also to be attributed to changes in the gradient outwards from the core of the arc or to bombardment effects. St. John observed a similar effect, and found displacements of 0·001, 0·002, and 0·004 Å.U. when arcs carrying currents of 5, 7, and 10 ampères respectively were increased by two ampères.

Some information regarding the effect of intensity upon the position of a spectrum line, (*f*), is contained in the discussion of the influence of pressure upon the arc spectrum of nickel already referred to. It appears that under pressure the displacement of faint lines is less than that of strong ones. For Group I. the increase in wave-length on the 1–10 intensity-scale is $2{\cdot}8 \times 10^{-4}$ Å.U. per unit of intensity, and for Group II. it is $3{\cdot}8 \times 10^{-4}$ Å.U. in the neighbourhood of 4000 Å.U. It is with some satisfaction, therefore, that we note that the intensity effect is not likely to be a serious disturbing factor in masking the relativity effect.

As far as (*g*), the influence of the surrounding atmosphere, is concerned, there is not much evidence which might enable us to arrive at a quantitative result, but the balance of evidence, obtained in an investigation of the nickel arc under pressure,* suggests that the displacement, to some extent at least, depends upon the partial pressure, and that an atom surrounded by foreign atoms does not suffer as large a displacement as when surrounded by atoms of its own kind. It is necessary to add that the evidence is not as precise as could be wished, and that other observers employing low pressures have not observed this effect. It points, however, to iron lines, when arising from an impurity in the nickel poles, being displaced at 80 atmospheres pressure about 15 per cent. less than iron lines given by a pure iron arc under the same external conditions. If we assume a linear relationship (of which, however, there is very inconclusive evidence) we should expect a 1 per cent. diminution in the displacement at 5 atmospheres, a value which is not likely to be exceeded by the solar pressure. The discrepancy thus calculated is 0·00034 Å.U. for the iron lines most sensitive to pressure. If the density of the iron vapour in the reversing layer were the same as that of the trace of iron in the nickel arc, we might expect a discrepancy of this order of magnitude in our observations, but it would not be comparable with the effect under test. But this evidence is slight, as we have no certain knowledge of the density of the material in the sun's absorbing envelope, and elements not so close in atomic number as iron and nickel might be expected to show larger effects.

As far as anomalous dispersion (*e*) is concerned, St. John,† after reducing the maximum outstanding effect, claimed by Julius to exist, to a question of 0·002 Å.U., upon close examination gives cogent

* Duffield, *Phil. Trans. Roy. Soc.*, A, **215**, p. 205, 1915.
† St. John, *Ap. J.*, **44**, p. 15, 1916.

and weighty reasons for rejecting the idea of any systematic displacements being due to this cause, a point of view with which Evershed agrees. With regard to (*h*), St. John* adduces evidence that, apart from a tendency on the part of a computer to introduce an error into his measurements of solar lines which are close together, the influence of neighbouring spectrum lines upon one another is without objective existence.

The Zeeman effect, (*l*), does not as a rule produce any large dissymmetry, and, except in sun-spots, the solar magnetic field is not likely to be sufficient to influence the position of a line in the spectrum. The writer cannot speak with certainty as regards the widths of the solar lines, (*n*), as given by the most modern instruments, but from the solar spectrograms in his possession, a few years old, it appears that few lines in the neighbourhood of 4000 Å.U. are as narrow as 0·05 Å.U. If we claim to measure to 0·001 Å.U., we assume that the position of maximum intensity is not displaced from its centre by 1/50th part of its own width. The whole Einstein effect would be equivalent to a displacement of about 1/6th of the width of the narrowest line.

Having now considered the general problem involved in measuring and interpreting the displacement of solar lines, it remains to discuss the possibility of obtaining results to the required degree of accuracy. Since any line which is displaced by pressure is liable to a great variety of influences which may affect its position, we may at once decide that it is unreliable as a means for testing the equivalence hypothesis. Of this class the symmetrically reversed lines of iron are least open to objection, but the fact that they are liable to pressure displacement lays them open to the suspicion of gradient influences in the solar atmosphere, whether regional effects have been observed in the arc or not. Unfortunately, therefore, little weight can be placed upon the extended examination by St. John† of 211 iron lines of the "stable" groups *a*, *b*, and *c*4 of the Mt. Wilson classification, whose mean displacement relative to the arc is +0·00038 Å.U., nor to the very interesting observation that the wave-lengths of the H and K lines in the sun agree so closely with those in the arc.‡ Nor can reliable inferences be drawn from the fact that 45 iron lines, which were found free from the pole effect, gave displacements for centre of sun − arc = +·0044 Å.U., and for limb of sun − arc = +·0150 Å.U. (Evershed and Royds, *Kod. Obs. Bull.*, No. 39).

It appears that the only way of rigorously testing the gravitational effect is to examine, as St. John and Evershed have done, the behaviour of lines which are not displaced by pressure.

This was the reason which led to the selection of the band spectrum at 3883 Å.U., usually assigned to cyanogen, which, even up to 100 atmospheres pressure, was found by Rossi not to be appreciably displaced.

* St. John, *Ap. J.*, **44**, p. 15, 1916.
† *Ibid.*, **44**, p. 311, 1916.
‡ *Ibid.*, **46**, p. 249, 1917.

It is worth considering this point a little more fully. Some bands undoubtedly are sensitive to pressure, for example, Rossi * found that the bands of the fluorides of calcium, barium, and strontium were displaced by pressure towards the red, and the writer † found a band in the silver arc which showed a displacement towards the violet. The question is whether the difference between the behaviour of these and of the CN bands is due to their production in the outer layers of the arc where their partial pressure is low, or whether there is some radical difference in the nature of the vibrating system (vibron), which gives rise to them, from that of the vibrons responsible for those bands which are sensitive to pressure.

I think the latter is favoured by the Zeeman effect, which is found to exist in the case of the fluorides of the alkaline earths, but which has not been found to influence these cyanogen bands. *As far as the evidence goes*, these bands should under no circumstances be displaced by an increase in the pressure or density of their vapour. This is not only of great importance in itself, but it also implies that their wave-lengths will not be affected by a pressure or density gradient, (*d*).

It thus seems that the most serious causes of displacement, (*a*) and (*d*), are ruled out, and it appears also that cyanogen has the further advantage of being unaffected by (*i*), (*j*), and (*k*), depending upon arc conditions. St. John examined its spectrum from the poles and centre of an arc and found no difference in wave-length. If this is truly the case, we could probably safely neglect (*m*) and the Stark effect (*l*), since these probably underlie the pole effect. Both St. John and Evershed consider it possible to select lines of the CN band free from the possibility of their being "blends" or close doubles, though they do not entirely agree as to the weight to be attached to the observation of individual lines, hence we may conclude that the disturbing cause (*h*) does not obtrude. With the cyanogen bands the intensity effect (*f*) is probably negligible, moreover (*e*), anomalous dispersion is not likely to be a serious disturbing cause, and the Zeeman effect (*l*) is non-existent, hence, provided that (*h*) is not a more serious matter in the solar spectrum than it appears to be in those of terrestrial origin, they appear to be satisfactory standards for testing the gravitational hypothesis. The invariability of the cyanogen wave-lengths, if true, is a very remarkable phenomenon; it is unexpected that the simplest type of atom, hydrogen, should be sensitive to pressure displacement and the complex cyanogen vibron insensitive. The writer confesses to a certain amount of doubt about the matter; under pressure the members of the cyanogen bands certainly broaden in the arc spectrum like symmetrical lines of pressure-sensitive elements, and their heads are subject to reversal, so it seems curious that they should differ in the one respect of showing

* Rossi, *Roy. Soc. Proc.*, A, 82, p. 518, 1909.

† Duffield, *Australasian Assoc. for Adv. of Science*, xii. p. 109, Brisbane, 1909.

no change in their centres of gravity; it is not as though pressure or density were totally without influence upon them.

Also with regard to the absence of a regional, or polar, effect in the arc, I think that there is a doubt whether the lines originate sufficiently close to the core of the arc to suffer a wide range of density changes, or sufficiently close to the poles to be subject to great variations in the intensity with which the vibrons responsible for them are bombarded. It is quite possible that they arise from interactions in the outer layers of the arc, in the quieter backwaters away from the main stream of the turbulent current effects, and that they have eluded the test. Further experiments upon their origin and their behaviour under different physical conditions are, I think, desirable. If we accept the invariability of the cyanogen wave-lengths, we see that a careful comparison between the arc and the sun should provide a means of separating out a quantity equal to $\{(b)+(c)\}$, and this is what Evershed and St. John have done in a variety of ways, of which the direct comparison method appears to the writer to be the most convincing. It is important to note, however, that (b) applies only to that portion of the sun upon which the slit of the spectroscope was set, and only to the particular day upon which observations were made.

Evershed and St. John have measured the displacements of the lines at the centre and the polar limb of the sun, the latter being the more important, because the *radial* currents in the solar atmosphere should not then affect the positions of the lines. This would not be legitimate if applied to lines affected by pressure, because the rays of light entering the spectroscope cut obliquely through the solar envelope, and consequently encounter different density gradients from those at normal incidence. But with the bands under discussion I think this is justified upon the available evidence.

It is important, however, to realise that we cannot simply by this means separate out the effects of line-of-sight motion from the gravitational effect, for we have little knowledge of the *surface* currents in the sun.* At the limb the spectroscope is sensitive to horizontal and at the centre to vertical solar currents, and that the former may be very appreciable we have the evidence of Pocock's observations, quoted by Evershed,† to the effect that the cyanogen spectrum from the south pole of the sun differed by ·004 Å.U. from that derived at the north pole during the period of observation. It is very important, as Evershed has pointed out, that the observations should be made over long periods and at different times, to obviate the influence of local solar currents. It might, I think, be useful to try a circular slit, with a diameter nearly equal to that of the sun, and to take the integrated light from the whole solar annulus instead of isolated samples from the polar regions.

If b_1 is the displacement at the centre due to motion, b_2 the displacement at the polar limb, and c, as before, the gravitational

* Duffield, "Meteorology of the Sun," *Qtly. Jnl. of Roy. Met. Soc.*, **41**, No. 175, p. 177, 1915.

† Evershed, *Observatory*, **41**, 531, Oct. 1918.

displacement, we see that Evershed and St. John measured the quantities $c \pm b_c$ and $c \pm b_l$.

The following are the available data* for the cyanogen band; it is assumed that corrections for earth motion have always been made, though this is not always specifically stated by the authors:—

	Centre – arc. $c \pm b_c$	Limb – arc. $c \pm b_l$	
Evershed and Royds†	+ ·006 (12)	+ ·008 (12)	
St. John	+ ·0006 (25)	·0 (17)	Weak lines.
	+ ·0024 (18)	+ ·0037 (17)	Stronger ,,
Evershed‡	+ ·004 (30)	+ ·006 (30)	
Pocock‡	+ ·004 (30?)	+ ·004 (30?)	N. Pole of Sun.
		+ ·004 ,,	Equator ,,
		+ ·008 ,,	S. Pole of Sun.

(The figures in parenthesis indicate the number of lines examined.)

The displacements of the individual lines show considerable amounts of disagreement among themselves, in spite of the fact that they are all attributed to the same vibrating system; for one or two of the members of the bands different observers agree in assigning negative displacements. This is rather suspicious, and suggests that even these very aloof cyanogen bands may be affected by some physical influence which has not yet been discovered.

Any conclusion which might be drawn from the above evidence would be stronger if the 43 lines examined by St. John and the 30 used by Evershed did not belong to the same type of radiating system. It would be preferable to obtain evidence from other pressure-insensitive bands if they can be found away from sun-spots.

To sum up, we may assert that the evidence on the whole shows a displacement in the direction demanded by the Einstein effect, but that the observed displacement is smaller than was predicted. Especially is this the case for St. John's investigation; Evershed's most recent work gives from half to three-quarters of the predicted displacement.

The question is how the general displacement to the red is to be interpreted. If the immutability of the cyanogen wave-lengths be conceded, is it more reasonable to regard the displacements as due to a general movement of the absorbing vapours upon the solar surface *away from* the earth at ·30 km. sec.$^{-1}$ at the centre and ·45 km. sec.$^{-1}$ at the pole (involving, according to Evershed, an "earth effect," though I doubt whether the cyanogen evidence demands such an extreme assumption), or to the full gravitational effect, together with about the same amount of movement (or

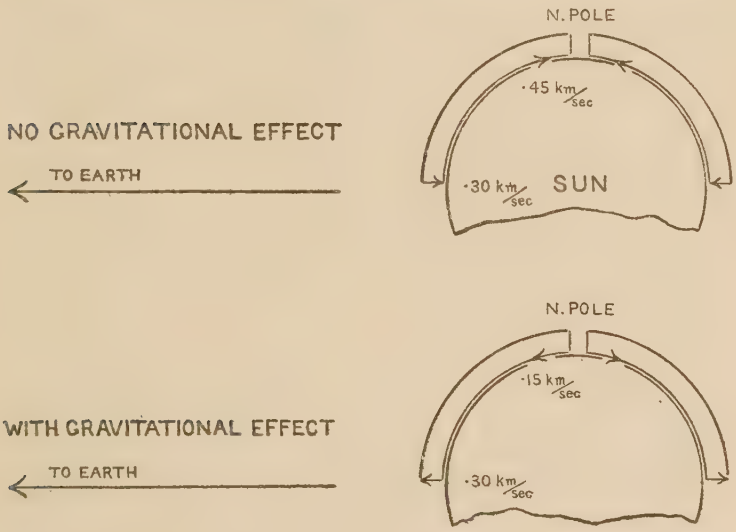
* See also note at end of paper.

† Evershed and Royds, *Kodaikánal Observatory Bulletin*, No. 39.

‡ Evershed, *Observatory*, **41**, 531, Oct. 1918.

rather less at the pole, according to Evershed's figures) of the vapours *towards* the earth?

I am not sure that the type of planetary circulation shown in the figure has been suggested to account for the general displacement of the solar lines to the red, but, if it could be set up in the sun, it should be capable of explaining the observed displacements at the centre and the poles; one can scarcely hope, however, that the general circulation, as distinct from local currents, is quite as simple as this, though it may be one factor. Equatorial limb currents are not dealt with, as the cyanogen evidence is not very definite. I have used the most recent figures given by Evershed,



the only reason I have for preferring them to those of St. John being that they were made later, when St. John's data were available for comparison. I have shown the currents as they would exist both if Einstein's hypothesis holds good and if it does not. There is some reason, I think, for expecting rising currents to be responsible for the absorption, and such slight evidence as solar meteorology affords * is rather more favourable to motion towards the equator at low levels. We may note that a velocity of ·15 km. sec.$^{-1}$ would carry vapour from the pole to the equator in about three months.

It is clear that we cannot speak with certainty upon the reality of the gravitational effect until we gain more knowledge of the meteorological conditions in the sun. Evershed † has recently observed the spectrum of sunlight reflected from Venus, and his

* Duffield, *Qtly. Jnl. of Roy. Met. Soc.*, **41**, No. 175, p. 177, 1915.
† Evershed, *Observatory*, **42**, Jan. 1919.

evidence favours an " earth effect," because, as the planet moves behind the sun, the sun-arc displacements change from positive to negative values. This seems at variance with the above suggestion, since circulation in the solar atmosphere should be symmetrical and the results from both sides of the sun the same. So far, however, Evershed's published results refer only to iron vapour ; further imformation concerning the cyanogen bands will be of interest, though, as the reflected light is integrated over the whole solar disc, we cannot hope therefrom to learn anything of local solar currents.

At present it is a case for suspense of judgment pending further inquiries into the invariability of the cyanogen bands, and further measurements by different observers of relative displacements in sun and arc for various parts of the solar disc of selected lines of cyanogen, and, if they exist, of other insensitive bands in other regions of the spectrum. These should, in my opinion, be made over a very extended period, certainly covering a sun-spot cycle, in case there is a change in the drift of the solar atmosphere during this period. I do not think that the evidence at present before us is sufficient either to affirm or to deny the equivalence hypothesis in so far as it depends upon a gravitational displacement of spectrum lines.

Physics Laboratory,
University College, Reading :
1919 *December.*

Note, 1920 *January.*—Grebe and Bachem (*Deutsch. Phys. Gesell. Verh.*, **21**, 454, 1919 July 30) have compared the solar and arc lines of cyanogen by a direct method, and find displacements for 36 lines of approximately ·004 Å.U., agreeing well with those of Evershed, and with a determination by Schwarzschild of ·0034 Å.U. They find no systematic differences at centre and limb. They claim to have discovered dissymmetry in the individual members of the cyanogen band, and deduce therefrom an increased solar displacement, which brings it to within 18 per cent. of that required by Einstein's hypothesis. In view of the probable existence of solar currents, I doubt whether the discovery of the full predicted displacement would be conclusive evidence in its favour ; the whole problem is inseparable from an examination of the solar circulation. Further, any dissymmetry of the cyanogen lines would cast serious doubts upon their use as criteria for such a test, for reasons which have already been discussed. In any case, the interpretation cannot be as simple as that given by Grebe and Bachem.—W. G. D.

On the Suggested Decrease of Period of Stars in Phillips's Group II., with Particular Notes on R Hydræ, S Tauri, U Herculis, R Aquilæ, χ Cygni, S Cygni, and S Coronæ Borealis. By H. H. Turner, D.Sc., F.R.S., Savilian Professor.

1. My attention was called to the star R Hydræ (4826) Chandler, *132422* Harvard, by Mr. C. L. Brook. In *H.A.*, **55**, 172–3, a period of 425 days is compared with the observations showing residuals of no meaning except for a short interval. There is no doubt that the period of the star is not constant, but has diminished considerably since it was discovered to be variable by Montanari in 1672. In his Third Catalogue of Variable Stars (Revision) Chandler gives the formula

$$2411931{\cdot}0 + 425{\cdot}15E - 0{\cdot}36E^2 + 15 \sin(7^\circ{\cdot}5E + 202^\circ),$$

and he proposed a formula of this kind in *A.N.*, **103**, under date 1882 Oct. But recent work has thrown doubt on the reality of periodic terms such as that above given, and a new determination of an independent kind seemed desirable.

2. I started with the modern observations (given in *H.A.*, **55**, as above) and worked backwards, before looking at any other references, in order to keep the determination quite independent; and I was led to the following hypothesis, which seems to work well:—

At regular intervals of 3415 days the star's period is *liable* to diminish, but does not always do so. When a change takes place, it is uniformly one of 10·00 days. This change has occurred eleven times since the variation was discovered, and the period has accordingly decreased from 518 days to 408 days.

3. It is not to be expected that these values (3415 days and 10·00 days) are accurate and final, but they allow of a close representation of the observations, and provide a working hypothesis simple enough to be worth trial for the immediate future. The numbers were only obtained after two or three trials, the first of which was very rough, but provided residuals systematic enough to suggest a second. This gave, even for the early observations, residuals small enough to provide a series of equations for solution, and the table below is the final outcome.

4. It was not until this table was obtained that I looked up the significance of the early observations, especially the two credited to Montanari, exactly two years apart (1670 April 15 and 1672 April 15), nor had I even read the note on p. 264, *loc. cit.*; so that it was with considerable satisfaction that I found that these were alternative suggestions for the same observation, and that my hypothesis had picked out the right year. Hevelius's observation also gives a residual close to that found by Chandler in one of his early hypotheses (*A.N.*, **103**, 321). That observation is not a true observation of maximum, belonging to the time when the star's

variation was not yet discovered; but since Hevelius saw the star, it must have been bright and was probably *near* maximum.

5. When there is a change of period, the following process has been adopted for assigning the next maximum. Take for instance the first case. When E = 6 the maximum is calculated to fall at 30823·50. A change takes place at 31110, *i.e.* 286·50 after the last maximum: so that 286·50/517·75 of the cycle has been run, leaving 231·25/517·75 to be run. But the period is now changed to 507·75 days, and on multiplying by this factor we get 231·25 − 4·47; or, taking the nearest quarter of a day, we get altogether

$$517{\cdot}75 - 4{\cdot}50$$

for this intermediate cycle. Generally, if P be the period before the change, P − 10 that after the change, and T the interval from maximum to change, the cycle including the change will take

$$P - 10(P - T)/P \text{ days} \quad \text{or} \quad (P - 10) + 10\ T/P \text{ days},$$

sometimes one form and sometimes the other being more convenient.

Fractions of a day less than one quarter have been neglected for convenience.

TABLE I.

Maxima of R Hydræ from H.A., 55, 172, compared with suggested Hypothesis.

Epochs when Period changed.	Period. d	E.	Calc.	Observed (*H.A.*, 55, 172).	O−C.
	517·75	1	28234·25	28201	− 33
		6	30823·00		
31110	507·75	7	31336·25	31120	−216
		8	31844·00	31851	+ 7
(7 missed)		31	43522·25	43512	− 10
		34	45045·50	45034	− 11
		60	58247·00		
58430	497·75	61	58748·25		
(3 missed)		87	71689·75		
72090	487·75	88	72185·50		
		89	72673·25	72678	+ 5
(1 missed)		90	73161·00	73163	+ 2
		101	78526·25		
78920	477·75	102	79012·00		
		105	80445·25	80447	+ 2
		108	81878·50	81877	− 2
82335	467·75	109	82355·75		
		115	85162·25	85160	− 2
		116	85630·00		

TABLE I.—*continued.*

Epochs when Period changed.	Period. d	E.	Calc.	Observed (*H.A.*, 55, 172.)	O - C.
85750	457·75	117	86090·25		
		119	87005·75	87004	− 2
(2 missed)		122	88379·00	88387	+ 8
		135	94329·75	94351	+ 21
		138	95703·00		
95995	447·75	139	96157·25	96148	− 9
		143	97948·25	97942	− 6
(1 missed)		144	98396·00	98386	− 10
		146	99291·50	99306	+ 14
		147	99739·25	99733	− 6
		148	00187·00	00188	+ 1
		149	00634·75	00638	+ 3
		151	01530·25	01525	− 5
		152	01978·00	01976	− 2
		153	02425·75	02422	− 4
02825	437·75	154	02872·50		
		156	03748·00	03742	− 6
		157	04185·75	04174	− 12
		158	04623·50	04612	− 12
		161	05936·75		
06240	427·75	162	06371·50	06359	− 13
		163	06799·25	06788	− 11
		167	08510·25	08513	+ 3
(1 missed)		168	08938·00	08943	+ 5
		174	11504·50	11514	+ 10
		177	12787·75	12798	+ 10
13070	417·75	178	13212·00	13226	+ 14
		179	13629·75	13642	+ 12
		180	14047·50	14047	0
		181	14465·25	14469	+ 4
		185	16136·25		
16485	407·75	186	16552·50	16565	+ 12
		187	16960·25	16960	0
		188	17368·00	17368	0
		189	17775·75	17770	− 6
		190	18183·50	18182	− 2
		191	18591·25	18595	+ 4
		193	19406·75	19415	+ 8
		194	19814·50	19821	+ 7
		195	20222·25	20226	+ 4

6. As an empirical formula the hypothesis works well, but it is not quite satisfactory from a physical standpoint.

In the first place, there is no indication why there should be changes of period at some multiples and not at others. If, for instance, changes occurred when the change-multiple fell within certain limits from the epoch of maximum, and there was no change when it fell outside them, the physical basis would be much strengthened. But not only is this not the case, but it does not seem possible to make it the case by any change of figures. With the figures as they stand, the change-unit 3415 days is 6·60 times the first period (517·75 days), 6·73 times the second (507·75 days), and so on, the series being

6·60 6·73 6·86 7·00 7·15 . . . 7·99 8·18.

(The increments begin with 0·13 and rise to 0·19. But for the purposes of the following argument they might be taken as uniformly 0·16.) Thus since the first change occurs at 31110 or E = 6·55, subsequent changes are *due* at values of E found by adding 6·73 successively, viz. at (6·55), 13·28, 20·01, 26·74, 33·47, 40·20, 46·93, 53·66 (60·39), but changes only actually occur for the two bracketed occasions. Now for the phase of the cycle we may disregard the integers and look only at the fractions. It is clear that with the adopted numbers we cannot justify changes at 0·39 and 0·55 with none at 0·47, which falls between them, and it is at once suggested that 3415 is not the right length of the change-unit. Let us try to find a suitable value. By way of illustration take a change-unit which is 6·95 times the second period. Then, starting with 6·55 as before, we have

(6·55) 13·50 20·45 27·40 34·35 41·30 48·25 55·20 (62·15),

which suggests that if we reserve the portion ·20 to ·50 for misses, the rest of the cycle may be devoted to changes. Now consider the interval to the next change, which contains three misses. The change-unit is now (say) 6·95 + 0·15 = 7·10 times the period, and changes are due at

(62·15) 69·25 76·35 83·45 (90·55),

which fits the second case of three misses: indeed, the figures were chosen to fit these two cases.

7. But immediately following this we ought to find one miss only. The change-unit is now 7·10 + 0·15 = 7·25 times the period, and we get

(90·55) 97·80 (105·05).

We must thus have a second reserve for misses near ·80, *i.e.* in the portion of the cycle nearly opposite to the former. This is beginning to complicate the matter, but there is nothing fatal so far. The next three are all changes followed by two misses, so that we have

(105·05) (112·45) (120·10) (127·90) 135·85 143·10.

But now we have ·10 both for a change and a miss, and ·45 for a change comes right in the portion reserved for misses.

8. This example will perhaps suffice to show the nature of the difficulties encountered. A good many trials have been made without success; and though there may have been some oversight, the conclusion suggested is that the determining cause between "change" and "miss" must be independent of the relation between period and change-unit.

9. The second unsatisfactory feature, from the physical standpoint, is the constancy of the ten-day diminution of period. It makes for simplicity, which is a recommendation in general; but we should associate with a physical cause a constant change rather in velocity (reciprocal of period) than in period. Probably the empirical formula could be rearranged on this basis, but we have scarcely enough to guide us at present, and it seems better to leave the above simple hypothesis as it stands.

10. R Hydræ is in Phillips's Group II., and its decrease of period is thus in accordance with the suggestion which came from the study of W Cygni, viz. that the long-period variables start life with long periods at the top of Group II.: that the period continually diminishes until it reaches something like two hundred days: that the star then crosses into Group I. at the short-period end and rises in Group I., *i.e.* its period lengthens again. There is nothing new in the recognition of the decrease in period of R Hydræ, which is expressed quite clearly by Chandler's term $-0{\cdot}36E^2$ quoted in § 1: the novelty in the present paper is the suggestion of diminution by constant steps at definite and regular intervals.

11. In Chandler's Third Catalogue (revised) there are the following stars with terms in E^2 and belonging to Phillips's Group II. [The stars of Group I. are reserved for examination in another paper.]

(1582) *S Tauri* $+ 380{\cdot}0E - 0{\cdot}15E^2$ (*Chandler*).

The Harvard number is 042309 and the observations are in *H.A.*, 55 compared with an adopted period of 365 days. A slight examination suffices to show that there has been one conspicuous change of period. It was at first 375·4 days, which involves renumbering the Harvard epoch 33 as 32.

TABLE II.

S Tauri 00080 + 375·4*E* to *E* = 36.

E.	Observed.	O−C.	E.	Observed.	O−C.
−3	98948	−6	32	12085	−8
−2	99326	−3	34	12848	+4
−1	99706	+1	35	13223	+4
0	00084	+4	36	13594	0

There is then a sudden change of period to 370 days, accompanied by a considerable jump in epoch. The period between E = 36 and 37 is only 347 days.

TABLE III.

S Tauri $13941 + 370{\cdot}0\,(E - 37)$.

E.	O.	O−C.	E.	O.	O−C.
37	13944	+3	42	15785	−6
38	14310	−1	43	16162	+1
39	14682	+1	44	16531	0
40	15048	−3	45	16905	+4
41	15421	0			

(5889) *U Herculis*, $411{\cdot}1E + {\cdot}0225E^2 - {\cdot}0035E^3$ (*Chandler*).

The coefficient of E^2 is positive, but the formula gives a *decrease* of period for values of E greater than 7, after an initial increase. The maxima tabulated in *H.A.*, 55, 194, give two changes of period (Harv. No. 162119).

U Herculis, Period 410·6 *Days.*

E.	O.	O−C.	E.	O.	O−C.
−16	00711	0	−10	03167	−8
−15	01109	−13	−8	03993	−3
−14	01538	+5	−4	05637	−2
−12	02359	+5	−3	06055	+6
−11	02764	0			

Period is then 404·5 *Days.*

0	07299	+2	6	09734	+11
1	07703	+2	12	12140	−10
2	08096	−10	14	12960	+1
3	08512	+2	15	13359	−4
4	08912	−3	16	13776	+9
5	09328	+9	17	14164	−8

Period is then 403 *Days, as in Harvard.*

19	14942	0	24	16948	−9
21	15769	+22	25	17363	+3
22	16149	−2	26	17759	−4
23	16543	−11	27	18167	+1

The initial date in each case is adopted to suit the observations which follow, and there are thus "jumps" at the breaks.

R Aquilæ 190108 (*Harv.*), 6849 (*Chandler*).

Chandler's formula is

$$2399163 + 355{\cdot}0E - 0{\cdot}63E^2 + 0{\cdot}005E^3.$$

There seems to be no doubt that for the first 20 maxima recorded in *H.A.*, 55, 209, the period was closely 345 days. There is a gap between 3 and 14 [J.D. 00220 and 04012] of 3792 days = 11 × 345 days (within 3 days). If again we take the outside maxima, 0 and 20 [J.D. 99167 to 06073], we have

6906 days = 20 × 345 days (within 6 days).

Adopting then a uniform period of 345·0 days for these 20 maxima, we get the following comparisons, taking the simple mean where observations are recorded from more than one source :—

TABLE IV.

R Aquilæ, Period 345 *Days.*

E.	O.	C.	O−C.	E.	O.	C.	O−C.
0	99167	174	− 7	15	04354	349	+5
1	99516	519	− 3	16	04685	694	−9
2	99861	864	− 3	17	05036	039	−3
3	00220	209	+11	19	05727	729	−2
14	04012	004	+ 8	20	06073	074	−1

The next observed maximum is at 11685, at a distance of

5612 days = 15 × 374 = 16 × 351 = 17 × 330 days.

Harvard has adopted 15 as the multiple, for some unexplained reason. It seems clear that the period must have changed in some way, and the next batch of maxima show a period near 330 days, as below. It is therefore natural to conclude that the change took place at E = 20 : for if we retain the longer period 345 days beyond E = 20, we should have to shorten the period *below* 330 days for the remainder of the interval between E = 20 and E = 37, and the run of the maxima from E = 37 to E = 49 shows that the period is not *below* 330 days. Carrying on therefore from the calculated date for E = 20, we get for

E = 37, 06074 + 5610 = 11684,

and continuing with 330 days, which also fits the gap from E = 42 to 47, we get figures as follows :—

TABLE V.

R Aquilæ, Period 330 Days: E = 20 to E = 47.

E.	O.	C.	O−C.	E.	O.	C.	O−C.
37	11684	684	0	50	15945	974	−30
38	12010	014	− 4	51	16271	304	−33
39	12339	344	− 5	52	...	634	
40	12683	674	+ 9	53	...	964	
41	13015	004	+11	54	17251	294	−43
42	13335	334	+ 1	55	17559	624	−65
47	14984	984	0	56	17885	954	−69
48	15298	414	−16	57	18212	284	−72
49	15635	644	− 9				

By E = 50 it becomes clear that another change of period has taken place, and the change seems to be rather earlier, say after E = 47. The Harvard table ends with E = 57, but the B.A.A. observations, partly published and partly put at my disposal very kindly by Col. Markwick and Mr. A. N. Brown, allow the changes to be followed up. The interval from E = 47 to 57 is 3228 days.

In view of the further change to 315·5 days, presently to be mentioned, it seems reasonable to adopt 322·75 days (exactly halfway between 330 and 315 days) as the next period, for comparison with B.A.A. observations.

TABLE VI.

R Aquilæ, Period 322·75 Days.

E.	B.A.A.	C.	O−C.	E.	B.A.A.	C.	O−C.
48	15302	306	− 4	56	17890	888	+ 2
49	15630	629	+ 1	57	18215	211	+ 4
50	15950	952	− 2	58	18550	534	+16
51	16270	275	− 5	59	18870	857	+13
52	16580	597	−17	60	19185	179	+ 6
53	...	920	...	61	19500	502	− 2
54	17230	243	−13	62	19815	825	−10
55	17570	566	+ 4	63	20130	148	−18

Up to about E = 59 the residuals show a slight increase, and we could improve them by increasing the adopted period a little—perhaps even to 324 days; but after that they fall quickly, and we clearly have another period. This was at first supposed to be 312 days = 24 × 13; and at my request Col. Markwick and Mr. A. N. Brown kindly supplied me with a beautiful set of readings at every 13 days, from the curves run through the B.A.A. observations. From such numerous points it is possible to determine the period by harmonic analysis of the whole curve, without limiting

attention to maxima only. Two sets of coefficients were obtained, one from the twelve odd numbers in each cycle, another from the twelve even numbers. To the phases deduced from the latter set 15° has been added to adjust them to the former. This seems to be for some reason not quite the right allowance, but the point need not delay us here. The results are otherwise in beautiful accord, as follows:—

TABLE VII.

R Aquilæ, Adopted Period 312 Days. Coefficients and Phases of the First Harmonics.

E.	Coefficients.		Phases.				
	Odd.	Even.	Odd.	Even.	Mean.	Calc.	M−C.
			°	°	°	°	°
59	2·5	2·5	233	235	234	247	−13
60	2·7	2·6	255	255	255	251	+ 4
61	2·7	2·7	253	255	254	255	− 1
62	2·5	2·5	261	257	259	259	0
63	2·6	2·7	270	255	262	263	− 1
64	2·6	2·7	268	255	262	267	− 5
65	2·5	2·6	275	264	270	271	− 1
66	2·7	2·7	278	278	278	275	+ 3
67	2·7	2·5	278	280	279	279	0
68	2·6	2·7	270	269	270	283	−13

An increase of 4° per cycle, corresponding to a period of 315·5 days, fits all the phases with such closeness that another decimal place could perhaps be used with advantage. The first and last are exceptions: probably the first is disturbed by the change of period in this neighbourhood, but this can scarcely be the case for E = 68 unless we have wrongly identified the epochs of change. For instance, it seems just possible that these epochs are at E = 7·5, 17·5, (27·5), 37·5, 47·5, 57·5, 67·5, etc., but this point can be more profitably discussed when further observations have thrown more light on the variation.

Summarising, we may give as a provisional account of this star—

TABLE VIII.

R Aquilæ.

E.	Max.	Period.
0	2399174	345·00 days
20	2406074	330·00 ,,
47	2414984	322·75 ,,
59	2418857	315·50 ,,
71?	2422643	

(7120) χ *Cygni*, $406{\cdot}0E + {\cdot}0075E^2$(194632 *Harv.*).

The positive term in E^2 denotes a small increase of period. The long series of residuals tabulated in *H.A.*, **55**, 218 to 220, against an adopted constant period of 406 days is seen at a glance to show an increase; but a very little examination suffices to raise doubts whether there is any permanent increase of *period*. The mean residual from E = 112 to 119 (years 1888 to 1896) is +133 days, and from E = 120 to 129 (years 1897 to 1907) is also +133 days, showing that the period was at the end of the nineteenth century closely 406 days, as it was during the eighteenth. Further examination shows that the observations can all be satisfied by the constant period adopted provided we admit four sudden changes of *phase* (*not of period*) at E = 85·5, 89·2, 92·9, 111·4, each change being of +28 days. It will be noticed that the change-epochs are separated by 1, 1, and 5 multiples of 3·7E = 1502 days. There is thus resemblance to R Hydræ in that these disturbances are liable to occur at regular intervals and are of constant amount, but non-resemblance in that the *phase* and not the *period* is disturbed. The observations given in *H.A.*, **55**, can be summarised on this hypothesis as follows: when several refer to the same maximum, the simple mean has been taken in each case, and fractions of a day are neglected.

TABLE IX.

χ *Cygni*. *Subtract Mean Residual* +20 *Days from Harvard.*

E.	O−C.	E.	O−C.	E.	O−C.	E.	O−C.
−69	+ 9	−43	− 4	+19	− 3	+71	−13
−68	+ 2	−42	+ 1	+20	−13	+72	− 1
−67	+ 2	−41	−12	+32	− 1	+73	+ 6
−66	0	−40	− 4	+47	+14	+74	+ 4
−63	− 5	−34	+16	+48	+16	+75	+12
−62	+ 3	−33	+ 7	+49	+26	+76	+ 3
−61	− 1	−28	+ 3	+52	+ 2	+77	+15
−56	− 2	−25	+ 9	+53	− 8	+78	+ 8
−55	− 9	−23	0	+54	− 6	+79	− 4
−52	0	−22	− 7	+55	− 6	+80	+ 3
−51	−13	−14	−19	+56	− 6	+81	+ 5
−50	−23	− 6	−39	+57	+ 4	+82	+10
−49	0	− 5	−44	+58	−12	+83	+11
−46	− 4	− 4	−57	+60	+ 8	+84	+10
−44	0	+18	+12	+70	−12	+85	+14

There are a few large residuals, notably those due to Le Gentil, at E = −6, −5, and −4; but the epoch returns to its former value, and any changes in either period or epoch are either mere fluctuations or must be of a different order of magnitude from those under

consideration. There is now a sudden jump of 28 days in epoch to 48 days.

Subtract Mean Residual +48 Days.

E	=	86	87	88	89
O−C	=	−2	+10	−3	−1

Then we have another jump.

Subtract Mean Residual +76 Days.

E	=	90	91	92	(93)
O−C	=	+1	+4	−2	+18

The maximum at E = 93 is disturbed by the next jump, which occurs at E = 92·9 on the supposition outlined above; it is thus intermediate between this set and the next.

TABLE X.

χ Cygni. Subtract Mean Residual +104 Days.

E.	O−C.	E.	O−C.	E.	O−C.
(93)	−10	99	+3	105	− 7
94	+ 4	100	−4	106	+ 5
95	0	101	−4	107	+ 9
96	+ 5	102	−3	108	+ 7
97	+ 1	103	0	109	+15
98	+ 2	104	+2	110	+20

Subtract Mean Residual +132 Days.

E.	O−C.	E.	O−C.	E.	O−C.
112	+11	118	+12	124	+ 1
113	− 4	119	− 4	125	− 9
114	−15	120	+ 3	126	+ 5
115	+ 2	121	− 2	127	+ 8
116	− 2	122	− 9	128	− 7
117	+ 8	123	+ 6	129	− 2

Some points of detail suggest further examination, but the main point seems clear, viz. that no *increase* of period is in evidence, apart from these advances by four sudden jumps in epoch.

(7220) *S Cygni* (200357 *Harv.*) $323E+0^{\cdot}015E^2$. *Large irregularity* (*Chand.*).

In *H.A.*, **55**, 222, maxima from E = −5 to E = +49 are tabulated against a uniform period of 323 days. They show considerable deviations, too large and systematic to be treated as accidental; but there are clear discontinuities near E = 26 and E = 36, and another less obvious near E = 14. We can make the intervals equal by selecting the epochs for possible change as

E = (2·3) 13·8 25·3 36·8 (48·3) 59·8 ?

In the cases bracketed there is no sudden change of phase. In the other three cases we can make the amount of change of phase the same without any undue straining of the facts, and in view of the experience of other stars it seems better to do so. We find at each discontinuity an accompanying change of period, as below :—

TABLE XI.

S Cygni, Period 322·5 *Days.*

E.	O.	C_1.	$O-C_1$.	E.	O.	C_1.	$O-C_1$.
−5	+30	+12	+18	7	+ 7	+6	+ 1
−4	+11	+11	0	8	+ 4	+5	− 1
−3		+11		9		+5	
−2	+ 4	+10	− 6	10	+ 4	+4	0
−1	+ 8	+10	− 2	11	+ 6	+4	+ 2
0	+11	+ 9	+ 2	12		+3	
1	+ 8	+ 9	− 1	13	+ 7	+3	+ 4
2	+10	+ 8	+ 2	14	−12	+2	−14
3	+ 9	+ 8	+ 1	15	+ 4	+2	+ 2
4	+ 8	+ 7	+ 1	16	− 6	+1	− 7
5	+ 8	+ 7	+ 1	17	−18	+1	−19
6	+ 6	+ 6	0	18	−26	0	−26

The quantity O is the simple mean of the residuals from a period of 323 days at the same epoch given in *H.A.*, 55, 222. The first observation at E = −5 is discordant, but there may be many reasons for that. Afterwards the values of $O - C_1$ are small until E = 14, when we assume a jump; but the series is continued a few terms to show its entirely different character. The observations after E = 13 are chiefly due to Knott, who was a careful observer and had been observing the star from the first in good accord with other observers, so that we cannot regard them as accidental. We therefore assume a sudden change as above stated.

TABLE XII.

S Cygni, Period 325 *Days.*

E.	O.	C_2.	$O-C_2$.	E.	O.	C_2.	$O-C_2$.
14	−12	−21	+ 9	22	− 8	− 5	− 3
15	+ 4	−19	+23	23	− 1	− 3	+ 2
16	− 6	−17	+11	24	+ 4	− 1	+ 5
17	−18	−15	− 3	25	+ 1	+ 1	0
18	−26	−13	−13	27	+28	+ 5	+23
19		−11		28	+20	+ 7	+13
20	−19	− 9	−10	29	+15	+ 9	+ 6
21	−15	− 7	− 8	30	+19	+11	+ 8

The residuals are not quite satisfactory, being positive at first and then negative. There may have been *two* changes of period, but one hesitates to multiply such changes without some warrant. Another suggestion may be hazarded : the star had been so regular up to E = 13 that the observer was probably taken aback by the sudden change, and unconsciously compensated it a little. Later he became reconciled to it, and perhaps overestimated it. After E = 25 there is again a sudden jump in phase, accompanied by a change of period.

TABLE XIII.

S Cygni, Period 321 Days.

E.	O.	C_3.	O−C_3.	E.	O.	C_3.	O−C_3.
27	+28	+23	+5	33	+12	+11	+ 1
28	+20	+21	−1	34	+ 9	+ 9	0
29	+15	+19	−4	35	+ 5	+ 7	− 2
30	+19	+17	+2	36	+ 3	+ 5	− 2
31	+12	+15	−3	37	+37	+ 3	+34
32	+13	+13	0	38	+25	+ 1	+24

There is clearly another break after E = 36.

Period 324 Days.

E.	O.	C_4.	O−C_4.	E.	O.	C_4.	O−C_4.
37	+37	+27	+10	47	+34	+37	− 3
38	+25	+28	− 3	48	+49	+38	+11
39	+16	+29	−13	49	+38	+39	− 1
40	+38	+30	+ 8				
41	+39	+31	+ 7				
42	+43	+32	+11				
43	+50	+33	+17	55	+36	+45	− 9
44	+36	+34	+ 2	56	+42	+46	− 4
45	+48	+35	+13	57	+48	+47	+ 1
46	+54	+36	+18	58	+59	+48	+11

The maxima from E = 55 to 58 have been estimated from MS. observations kindly supplied by Mr. C. Grover of the Rousdon Observatory, in order to check the continuity of the period after E = 48·3, which the single maximum at E = 49 left in some doubt. The star has apparently not been on the list of the B.A.A., and observations at Rousdon were discontinued for some years after the maximum at E = 49. Fortunately they were resumed (on Mr. Grover's own initiative) at E = 55, in time to provide the above valuable check. The period has been adopted so as to satisfy these

four maxima at one end, and at the other to give a jump of twenty-four days as at E = 13·8 and 25·3. The residuals are not quite satisfactory, being rather too systematically positive in the middle, but the material scarcely warrants much greater refinement at present.

We thus have the following hypothesis:—

E.	Period.	Abrupt Change in Epoch.	Period.
	d	d	
−5 to 13·8	322·5	−24	
13·8 ,, 25·3		+24	325·0
25·3 ,, 36·8	321·0	+24	
36·8 ,, (59·8)?			324·0

which is consistent with a secular decrease of period accompanied by oscillations. We have the precedent of S Herculis and R Leonis for this kind of behaviour, and that of W Cygni for jumps in epoch in either direction.

One further point may be noticed: after one of the abrupt changes the residuals are in general positive at first and then negative. We noticed this above at E = 14 as possibly due to the observer's unconscious rebellion against the change; but it may be due to the star—a feature of the disturbance.

To the above stars, chosen because of the terms in E^2 assigned by Chandler, one more may be added (S Coronæ) for a quite different reason:

S Coronæ Borealis, 151731 *Harv.* (5504) *Chandler.*

Chandler gives the formula

$$2400647 + 361{\cdot}2E + 8 \sin (12^\circ E + 327^\circ).$$

In discussing the Rousdon observations a period of 363·3 days was deduced (*Mem. R.A.S.*, **55**, 83) which, compared with Chandler's (earlier) 361·2 days, seemed to indicate an increasing period. As S Coronæ belongs to Phillips's Group II., this seemed to be a case unfavourable to the hypothesis that the periods of Group II. stars are decreasing. The observations were therefore specially examined. Mr. Grover had kindly put into my hands the Rousdon observations up to 1918, and these were carefully analysed by Mr. F. Sargent, to whom my thanks are due. His analysis showed a period of 361·5 days for the years 1901–1919, so that there was no confirmation of a permanent increase to 363 days. The residuals given in *H.A.*, **55**, 185, were then examined, and it was seen that there had been two sudden changes of epoch, the second of which gave the illusory 363-day period. The facts are now interpreted as follows. The column "Harv." gives the Harvard residual from a uniform period of 361·2 days.

TABLE XIV.

S Cor. Bor., Period 362 Days.

E.	Harv.	Corrd.	E.	Harv.	Corrd.
0	+ 8	+15	10	− 7	− 8
4	+ 3	+ 7	11	− 1	− 3
5	−18	−15	12	+19	+16
6	− 5	− 3	13	0	− 3
7	+12	+13	14	0	− 4
8	+ 1	+ 2	15	+ 3	− 2
9	− 5	− 5	16	+10	+ 4

There is then a jump of 16 days in epoch, and a change of period to that adopted, viz. 361·2 days. The Harvard residuals thus require simply a constant correction of +10 days, which is +16 days different from the correction of −6 days applied at E = 16.

TABLE XV.

S Cor. Bor., Period 361·2 Days.

E.	Harv.	Corrd.	E.	Harv.	Corrd.
17	−15	− 5	26	− 6	+ 4
18	− 6	+ 4	27	−17	− 7
19	− 8	+ 2	28	−11	− 1
20	− 3	+ 7	29	−15	− 5
21	− 5	+ 5	30	+11	+21
22	−20	−10	31	− 9	+ 1
23	−15	− 5	32	−22	−12
24	−14	− 4	33	− 6	+ 4
25	− 8	+ 2	34	+13	+23

After E = 33 there is a jump of equal amount in the other direction and the period changes towards 362 days again: but it seems to be distinctly less than 362·0. Mr. Sargent estimated twenty maxima, the mean of the first ten being at J.D. 2416736·4 and of the second ten at 2420351·4 : interval 361·50. The interval between his first and last gives 361·80. Inspection of the Harvard residuals suggested a correction of 2/3 of a day to the adopted 361·2, or a period of 361·87. We will adopt a correction of *half* a day to Harvard, *i.e.* 361·7 days. The residuals then become—

TABLE XVI.

S Cor. Bor., Period 361·7 Days.

E.	Harv.	Corrd.	E.	Harv.	Corrd.
34	+13	+ 7	42	+24	+14
35	+ 7	+ 1	43	+10	0
36	− 7	−14	44	+14	+ 3
37	+14	+ 7	45	+10	− 1
38	+ 9	+ 1	46	+35	+23
39	+ 2	− 6	47	+22	+10
40	+20	+11	48	+15	+ 2
41	+10	+ 1			

A rather longer period would suit these better, but the maxima estimated by Mr. Sargent would give the following residuals:—

E.	Rousd.	E.	Rousd.	E.	Rousd.
40	+ 1	46	+ 6	53	+ 5
41	0	47	+25	54	−11
42	+ 3	48	−28	55	+52
43	+ 9	49	+21	56	−33
44	+20	50	−21	57	−10
45	− 2	51	+23	58	+15
		52	+ 8	59	+ 4
Mean	+ 5		+ 6		+ 3

The means of these three groups show that the adopted period is approximately right. A more elaborate discussion will be made when the Rousdon observations come to be published.

As already remarked, the spurious period of 363 days formerly obtained from the Rousdon observations was due to not recognising the jump of 16 days between E = 33 and E = 34. If we refer to Table xl. on p. 83 of the memoir cited, and determine the correction to epoch from each cycle separately, we get, for the first six cycles,

$$-9, -1, -17, -1, -11, -9\text{; mean } -8 \text{ days,}$$

and for the next eight cycles,

$$+3, +6, -18, +8, +25, +15, +1, +19\text{; mean } +8 \text{ days.}$$

The difference of 16 days was erroneously ascribed to a change of period, whereas it takes place suddenly between E = 33 and E = 34. On the present interpretation there was also an increase of period of about half a day, but there had previously been a greater decrease of 0·8 day. We may now examine a little more closely whether the decrease was actually *greater* than the increase. The early observations are closely fitted by a period 362·0 days if we exclude the first, for which there may be two reasons: viz. it may be rough, being the first; and it may be affected by another "jump" which is due about this time. Excluding it, the sum of the residuals (corrected) from 4 to 9 is − 1, and of those from 10 to 16 is zero.

If we include the first residual + 15, we get a decrease of 2·0 days in 7·5 cycles, or ·27 per cycle, making the period 361·73 days, which is practically the third value. At present, therefore, the evidence is insufficient to establish a decrease—it is consistent with a return to the original value. But there is no reason to suspect any *increase*.

Summary.

All the stars of Phillips's Group II., which are credited with a secular change of period in Chandler's Third Catalogue (Revision), have been examined. In no case does there appear to be a *secular*

increase of period, though in two cases this was suggested by Chandler. In one of these (χ Cygni) the period is sensibly constant, but there have been four sudden changes of phase ("jumps"), all of +28 days approximately, and all separated by multiples of a constant interval. In the other case (S Cygni) similar jumps are accompanied by changes of period equivalent to a secular decrease with fluctuations.

R Hydræ has long been known to have a conspicuously decreasing period, but a new law of decrease in equal steps is now suggested.

In addition the star S Coronæ Borealis, which (in the paper on W Cygni) was suspected of being an exception to the rule, has been specially examined and found to be not inconsistent with it.

The stars in Group I. will be examined in another paper, but a preliminary examination shows that their periods are increasing, with one or two possible exceptions.

The Longitude of the Moon from 1627 *to* 1918. By J. K. Fotheringham, M.A., D.Litt.

Hansen's *Tables de la Lune*, published in 1857, had the merit of reducing to a fraction of their former amount the unexplained fluctuations in the Moon's motion. All that has been done since then in the way of applying gravitational theory to the discovery of unknown inequalities or to the more exact determination of known inequalities has accounted for a small part only of the outstanding discrepancy between theory and observation. Hansen's work may, therefore, be taken as the starting-point of the problem of the Moon's longitude in its present form.

Hansen derived the inequalities in the Moon's motion primarily from theory, but in the case of the larger of his two Venus inequalities he would appear to have altered the phase and, perhaps, the coefficient in order to represent the Greenwich observations from 1750 to 1850. For the secular acceleration he depended on ancient and mediæval solar eclipses. The epoch and mean motion as arbitrary constants of the theory could be determined from modern observation only. The larger of the two Venus inequalities, to which Hansen assigned a coefficient of 21″·47, has since been found to possess a coefficient of 0″·24 only. Its period is 239 years. It may, therefore, be said that Hansen's expression for the longitude of the Moon does in effect include a large empirical term with this period. It will be noted that every subsequent solution of which details have been published includes a similar empirical term.

The next step of importance was made by Newcomb. In his *Researches on the Motion of the Moon*, Part I., published in 1878, he discusses and briefly dismisses a few ancient and mediæval solar eclipses. He discusses the times of all the lunar eclipses mentioned by Ptolemy in the *Almagest*, and also handles a large number of

mediæval lunar eclipses. But the chief value of the work for the present purpose lies in its diligent collection and reduction of observations of occultations and solar eclipses made between 1621 and 1747. During the earlier part of this interval the weight falls mainly on the eclipses, during the latter part on the occultations. Accepting Hansen's conclusion that his tables satisfied the Greenwich meridian observations from 1750 to 1850, and adding the correction obtained from the occultations observed about the epoch 1875·0, Newcomb was able to use a series of observations spread over 250 years, thus having a great advantage over Hansen, who had been content with the 100 years over which the Greenwich meridian observations had extended. The whole material, ancient and modern, was used to determine the Moon's epoch, mean motion, and acceleration, and, as the result clearly indicated the existence of an unexplained long-period term, the modern observations were solved separately to determine this term and at the same time to correct further the epoch and mean motion. Like Hansen, Newcomb did not deduce the period of his empirical term from the observations, but assumed that it was identical with that of one of Hansen's Venus terms, in this case the smaller, for which Hansen had calculated a period of 273 years, reduced by more recent investigations to 271 years. The coefficient and phase of this term were determined from the observations, and the agreement seemed satisfactory in view of the necessary imperfection of the theory. The total effect of these corrections was to reduce Hansen's mean motion for epoch 1800 by about 29″ and his acceleration from +12″·2 to +8″·3 per century, and to substitute an empirical term with a period of 273 years and a coefficient of 15″·5 for his faulty Venus term, which, as has been seen, had a period of 239 years and a coefficient of 21″·47. The corrections thus obtained have been used in the *Nautical Almanac* from 1883 onwards. It is no small tribute to the thoroughness of Newcomb's work that while the observations from 1750 onwards have been thoroughly examined by Cowell, Radau, and Brown, the reduction of the observations from 1621 to 1747 has never been revised by anyone except Newcomb himself.

In 1904 Dr Cowell contributed to *Monthly Notices* * a paper dealing with the empirical terms of long period in the Moon's longitude, based on an analysis of the Greenwich meridian observations from 1750 to 1901. Unlike Newcomb, Dr. Cowell attempted to satisfy the observations rigorously and found that this required three empirical terms. Two of these could be determined satisfactorily from the meridian observations on the assumption that there was to be one and only one term of very long period. The two terms came out with periods of 66 years and 42 years respectively and with coefficients of 2″·35 and 0″·73. But it was found that the principal empirical term could not be determined from the Greenwich series alone. Dr. Cowell therefore adopted such a value as would with the necessary corrections to the secular terms

* 65, pp. 34–53.

and with the two minor empirical terms satisfy the meridian observations rigorously, and at the same time give for the interval from 1621 to 1750 a result differing as little as possible from that obtained by the use of Newcomb's secular and empirical terms. He found that, if Newcomb's value for the acceleration was to be retained, the desired result could best be achieved by applying a correction of $-4''\cdot3$ to Newcomb's mean longitudes and giving the large empirical term a period of 363 years and a coefficient of $17''\cdot86$. At the same time he showed how an equally good solution could be obtained for any other assumed value of the secular acceleration. The permanent contributions to the subject in this solution were (1) the recognition of minor fluctuations, (2) the abandonment of Hansen's and Newcomb's attempts to represent the fluctuations by a mere change in the coefficient and phase of known gravitational terms, and (3) the demonstration of the limits within which the solution could be regarded as unique.

Although no determination of the minor fluctuations had been published by any previous investigator, Mr. Nevill had, as it turns out, already recognised them, and had attempted to find a satisfactory expression. In *M.N.*, 65 (1905), p. 267, he stated that he had twenty years earlier adopted a term with a period of 80 years and a coefficient of $2''\cdot80$ in order to satisfy the Greenwich observations made since 1750, but had afterwards replaced it by two terms with periods of 129 and 88 years respectively, and with coefficients of $1''\cdot95$ and $2''\cdot69$. By so doing he believed he had succeeded in satisfying the older observations, and had at the same time satisfied the Greenwich series more closely. A period of 82 years can, in fact, be clearly traced in the residuals published by Newcomb in the first part of his *Researches*, p. 267. Here we find maxima indicated for 1698, 1780, 1862·5, and minima for 1657, 1737·5, 1826. The apparent coefficient is, of course, affected by the method of least squares, and appears with a small value where the equations have a heavy weight attached to them, and with a larger value where the equations receive less weight. Altogether the coefficient which can be deduced from Newcomb's residuals varies from $0''\cdot5$ to $2''\cdot5$.

The most striking feature of Dr. Cowell's solution is the length of the period which he assigns to the principal empirical term as compared with the same period as determined by other investigators. Dr. Brown has devoted a paragraph to the subject in his paper on the "Longitude of the Moon from 1750 to 1910" (*M.N.*, 73 [1913], pp. 710, 711). Dr. Brown, who regards Dr. Cowell's term as a deduction from the Greenwich observations from 1750 onwards, points out that from the same observations he himself, using Newcomb's periodic term in its revised form, which gives it a duration of 275 years, has always obtained a positive correction to Newcomb's acceleration. Now, as Dr. Cowell found, the smaller the value adopted for acceleration, the shorter the period. It follows that, if the acceleration is to be reduced to Newcomb's or

a smaller value, the period must be reduced below Newcomb's value. He suggests that the difference between the results is mainly due to his own new corrections to the Moon's place, though he thinks it may in part be due to a difference in the mode of analysis. But it is clear from Dr. Cowell's paper* that he found from the Greenwich series, after removing the minor periodic terms, a correction of $+1''\cdot6$ to the coefficient of the square of the time as expressed in his units of about $45\frac{1}{3}$ years, which would be equivalent to a correction of $+8''\cdot0$ to the same coefficient, when the time is expressed in centuries, but he held that it was impossible to determine how much of this correction was to be assigned to the acceleration, and how much was to be regarded as part of the expression of the large empirical term in powers of the time. Even if the acceleration were supposed to be known, he held that it was impossible to determine how far this correction should be represented by a change in the coefficient of the cosine in the expression of this inequality, and how far by a change in the period. His conclusion was, therefore, an anticipation of the similar conclusion which Dr. Brown makes in the paragraph following that to which I have referred.

Dr. Cowell's value for the period of the large empirical term does in fact depend on the combination of Newcomb's reduction of eclipses and occultations before 1750 with his own reduction of the meridian observations after that date, and he has, as stated above, adopted that value which, when combined with the necessary corrections to the secular terms, gives the least deviation from the result obtained by Newcomb's formula for the interval from 1621 to 1750, consistent with the rigorous satisfaction of the observations since 1750. In other words, Dr. Cowell preferred to make any necessary sacrifice of accuracy in satisfying the observations before 1750 rather than abandon anything of the rigorous accuracy with which his formula satisfied the observations since 1750. The maximum difference between his solution and Newcomb's fell in the neighbourhood of 1690, where his formula gave the Moon a longitude about 5″ in excess of that given by Newcomb's formula. A reference to the final group residuals published by Newcomb on p. 270 of the first part of his *Researches* will show that in 1699·7 the observed place was $5''\cdot0$, and in 1706·4 $7''\cdot5$ in excess of the place found by his formula. It would appear, therefore, that Dr. Cowell was, on the evidence before him, perfectly justified in supposing that Newcomb's theory admitted of a positive correction of 5″ about 1690, and he would have been wrong in sacrificing his rigorous satisfaction of the meridian observations in order to approximate more closely to Newcomb's solution of the earlier observations.

But, while it appears that Dr. Cowell's was the best possible solution on the evidence before him, it is clear that, if Newcomb had marshalled the evidence differently, the weakness of Dr. Cowell's solution would have been apparent. The residuals for

* *Ubi supra*, pp. 52, 53.

1699·7 and 1706·4 are each taken from a single solar eclipse. Occultations in and around these years are numerous, but Newcomb chose to include the occultations down to 1690 in a group with mean epoch 1681·7 and those from 1699 onwards in a group with mean epoch 1713·1. These groups, which receive weights many times greater than the eclipses, yielded residuals of $+1''\cdot3$ and $+0''\cdot5$ respectively, and lend no colour to the idea that a correction of $+5''$ to the combined value of Newcomb's secular terms and large empirical term is possible near this date. In the second part of his *Researches*, in which Newcomb rejected all eclipses later than the year 1666, he was able to show that seven successive groups, covering the observations from 1658 to 1757, yield residuals of less than $1''$. For the two epochs 1699·7 and 1706·4 Newcomb's later formula gives, so far as secular and long-period terms are concerned, longitudes $0''\cdot30$ and $0''\cdot34$ less than his earlier formula, and it is clear, therefore, that Dr. Cowell's formula gives a result greatly exceeding the permissible deviation from Newcomb's result for the interval in question. The conclusion is that a rigorous satisfaction of the observations since 1750 is not possible or is only possible on more complicated assumptions than those made by Dr. Cowell.

In 1909 Newcomb published in *Monthly Notices*, 69, pp. 164–9, his paper entitled "Fluctuations in the Moon's Mean Motion," in which he gave the results of a new discussion of material used in the first part of his *Researches* down to 1747, supplemented by the occultations from 1753 to 1908. This paper contained the new value for the large empirical term, which was now given a period of 275 years and a coefficient of $12''\cdot95$, and the residuals of groups of observations were given from 1621 to 1908. The paper also contained a valuable general discussion, which was not repeated in the larger work which it heralded. The new values for the secular terms were published in a subsequent paper the same year, "Comparison of Ancient Eclipses of the Sun with Modern Elements of the Moon's Motion," *M.N.*, 69, pp. 460–7, in which the acceleration was fixed at $+8''\cdot0$.

In 1911 Radau's *Tables de la Lune* (Annales du Bureau des Longitudes, vii.) appeared, containing on pp. [69] to [93] of the Introduction a discussion of the empirical inequalities. In addition to the ancient and mediæval eclipses, Radau used the results of the first part of Newcomb's *Researches* for the errors in the Moon's place from 1621 to 1728, and the Greenwich observations for the Moon's place from 1751 to 1910. In addition to corrections to the secular terms, including a diminution of the acceleration to $+7''\cdot7$, he found a term with a period of 267 years and a coefficient of $11''\cdot5$, and a term with a period of 64 years and a coefficient of $3''\cdot3$. Radau's corrections have been used along with the rest of his tables in *Connaissance des Temps* since 1915.

Newcomb's *Researches on the Motion of the Moon*, part ii.,* was published posthumously in 1912, and contained with much other work the detailed reduction of the occultations from 1672 to

* *Astronomical Papers of the American Ephemeris*, ix., part i.

1908, and a full *exposé* of the method by which the observations were worked up.

In 1913 Dr. Brown published in *Monthly Notices*, 73, pp. 692–714, a paper entitled "The Longitude of the Moon from 1750 to 1910." This contains the corrections required to reduce Newcomb's and Cowell's places to Dr. Brown's theory including Newcomb's empirical term.

These are followed by a detailed comparison of the results both of the meridian observations and of the occultations with the theory thus revised. Dr. Brown does not attempt to deduce from the observations a new value of the empirical term, but first modifies Newcomb's secular terms to compensate for his own corrections to the gravitational terms of long period, and then transforms both the secular terms and the empirical term to compensate for a reduction of the acceleration to its theoretical value. These two transformations were revised in a subsequent paper published in 1915, on the "Elements of the Moon's Orbit," *M.N.*, 75, pp. 508–16, where the results form part of the final values on which Dr. Brown's Tables of the Moon are being constructed. With the value finally adopted for the empirical term the period is given as 257 years, and the coefficient is reduced to 10″·71. The minor empirical terms were not determined in these papers.

Separate determinations of the minor empirical terms have been published by Dr. Ross, *M.N.*, 72 (1911), Professor Dale, *M.N.*, 74 (1914), pp. 642–6, and Mr. Tustin, *A.J.*, 31 (1918), pp. 105–7, all based on the occultations only. Dr. Ross finds a period of 57 years with a coefficient of 2″·9, and a period of 23 years with a coefficient of 0″·8. Professor Dale finds a period of 56 years with a coefficient of 2″·96, and a period of 19 years with a coefficient of 1″·11. The latter, though empirically determined, is, as Professor Dale points out, nothing else than the gravitational correction to Newcomb's value for the term which contains the longitude of the node in its argument. Mr. Tustin finds three terms, of which the largest has a period of 63 years and a coefficient of 7″·3. All three solutions assume the correctness of the combined effect of Newcomb's values for centennial motion, acceleration, and the large empirical term.

Newcomb's study of the occultations was continued by Dr. Ross to 1913 in the *Astronomical Journal*, 28 (1914), pp. 153–6, and to 1915 in the same journal, 29 (1915), pp. 65–8, and by Mr. Newton to 1917 in the same journal, 31 (1918), pp. 97–100. Dr. Brown compared the Greenwich observations to 1915 with his theory in *A.J.*, 30 (1917), pp. 176–8.

In the earliest of these papers Dr. Ross pointed out the probability of Newcomb's long-period term breaking down, but I am not aware of any attempt to replace it by a term which will do better justice to the more recent observations.

In the present paper I have adopted Newcomb's work as a basis, not because I consider the occultations more reliable than

the meridian observations, but because Newcomb had published his work in great detail and it seemed the simplest course to use it as a basis for my own. The present investigation differs from Newcomb's in that I (1) confine my attention to observations made since the invention of the telescope, (2) exclude the solar eclipses, (3) add the Greenwich observations for the years 1908–1918, (4) adopt Dr. Brown's values for terms of long and mean period.

(1) The ancient and mediæval eclipses have little value except for the determination of the acceleration; their value for the present purpose is vitiated by the probability that the Sun's motion is also subject to acceleration; and the discussion would present few points of analogy to that of modern observations. I consider that these eclipses are best discussed separately, and that alternative solutions should be attempted for the modern observations, which will show what secular terms and what constants of the empirical term cohere with each of a number of assumptions as to the period of the large empirical term. These solutions will at the same time show what other terms cohere with any assumed value for the acceleration.

(2) The solar eclipses from 1621 to 1666, reduced by Newcomb in the first part of his *Researches* with Hansen's Tables of the Sun and used by him in the second part of his *Researches*, yield longitudes for the Moon exceeding in the mean by $+5''\cdot5 \pm 5''\cdot6$ the longitudes resulting from the occultations within the same limits of time. This difference is doubtless due in part to errors in Hansen's secular terms in the Sun's motion, partly to errors in his values for the theoretical inequalities of the Sun, and partly to unexplained fluctuations in the Sun's longitude. An instructive table of these last from 1750 onwards is given by Dr. Ross in his "New Elements of Mars."* There seems good reason to believe that they are connected with the fluctuations in the Moon's longitude. It seemed best, therefore, to exclude the solar eclipses altogether.

In forming equations of conditions, I have adopted as a basis the groups, mean corrections, and probable errors given by Newcomb on p. 206 of the second part of his *Researches*. As the groups down to mean epoch 1662 consisted partly or wholly of eclipses, it was necessary to substitute for these other groups consisting wholly of occultations. I have, therefore, adopted for the first group the observations by Gassendi and Bullialdus discussed on p. 231 of the first part of the *Researches*, and for the second and third groups the two first groups of observations by Hevelius discussed on p. 232 of that work.

The corrections to Hansen deduced by Newcomb from each of these groups are shown in column *a* in Table I. of this paper. It should be observed that Newcomb has substituted for the quantities contained in Hansen's Tables xxii. to xxxviii. the sum of their constants, thus producing a probable deviation of

* *Astronomical Papers of the American Ephemeris*, ix., part ii. (1917), p. 261.

$\pm 2''\cdot7$ from the rigorously computed longitude in the case of each separate night's observations.*

The first of these groups contains thirteen occultations ranging from 1627 to 1638. Had I noticed before completing my computations that the observation of 1632·1 was of Mars, not of a fixed star, I should have excluded it. The exclusion of this observation would give epoch 1636·1 for the group and $+59''$ for the correction to Hansen. I do not know how Newcomb derived the probable error. Computed in the ordinary way, it should be $\pm 7''$ whether we include the observation of Mars or not. I have retained Newcomb's value, $\pm 9''$.

For the second and third groups Newcomb, after finding probable errors of $\pm 8''$ and $\pm 4''$ respectively, raised these quantities to $\pm 10''$ and $\pm 5''$ on the ground that the four groups of observations by Hevelius had an accidental agreement, which contrasted with the discordance of the individual observations. Since Newcomb in the second part of his *Researches* decided not to use the two later groups of observations by Hevelius, I see no reason for retaining his additions to the probable errors. The adopted probable errors of the three groups are set out in column *e* of Table I.

The observations were reduced to Dr. Brown's theory, so far as secular and long-period terms are concerned, by the application of (*b*) the reductions tabulated by Newcomb on pp. 30 and 203 of the second part of his *Researches*, and (*c*) and (*d*) the corrections necessary to reduce Newcomb's secular terms and theoretical terms of long period respectively to Dr. Brown's values. The mean longitude resulting from the application of (*b*) is, apart from the unimportant coefficient of T^3, the same as that which is now used in the *Nautical Almanac* and is expressed by the formula

$$\lambda = 335^\circ\ 43'\ 25''\cdot56 + (1336^{\text{rev.}} + 1108390''\cdot44)T + 9''\cdot541T^2 + 0''\cdot0068T^3,$$

where T is the time in Julian centuries from 1800 Jan. 0·0. To this Dr. Brown † applies the correction

$$\Delta\lambda = +2''\cdot25 + 1''\cdot36\,T - 2''\cdot42\,T^2.$$

Dr. Brown's correction to the theoretical terms of long period is to be found in *M.N.*, 73, pp. 701, 702. It reduces to the form :—

$$\begin{aligned} &+0''\cdot84 \sin(20^\circ\cdot2T + 31^\circ\cdot0) \\ &+0''\cdot31 \sin(16^\circ\cdot2T + 68^\circ) \\ &+0''\cdot28 \sin(133^\circ T + 32^\circ\cdot4) \\ &+0''\cdot04 \sin(6^\circ T + 308^\circ). \end{aligned}$$

The outstanding correction to the tabular place after the application of reductions *b*, *c*, *d* is shown in column *n* in Table I. It is the total correction to be applied to Dr. Brown's tabular

* *Researches*, etc., part i., pp. 189, 195.
† *M.N.*, 75, 510.

places when freed from the empirical term, ignoring Dr. Brown's corrections to terms of short and mean period.

TABLE I.

Epoch.	*a*.	*b*.	*c*.	*d*.	*n*.	*e*.	Weight.
1635·7	+57″	−56″·37	+6″·51	−0″·16	+7″	±9″	$\frac{1}{81}$
1645·2	+33·6	−55·80	+5·66	−0·15	−16·7	±8	$\frac{1}{64}$
1662·0	+38	−51·43	+4·24	−0·10	− 9·3	±4	$\frac{1}{16}$

Newcomb's groups with mean epochs 1681 to 1900 require slightly different reductions. For these Newcomb gives us, on p. 206 of the second part of his *Researches*, what profess to be "observed mean corrections to the purely theoretical longitude of the moon" in the shape of "mean results from the smoothed-off means of § 46" (*i.e.* pp. 195, 196). These are supposed to include not only the reductions tabulated on pp. 30 and 203 of the second part of the *Researches*, but also the reductions necessary in order to correct Hansen's errors in terms of short and mean period. Newcomb has, however, been guilty of a clerical error in describing the corrections as mean results from the smoothed-off means of § 46. The figures he gives are in fact mean results from the unsmoothed means of §§ 41, 45 (pp. 127, 193, 194). If we assume that the means of p. 206 are unweighted arithmetical means of the means of §§ 41, 45 for quantities varying uniformly with the time, we shall obtain results differing in no case by more than ±0″·2 from those tabulated by him. I have decided to regard Newcomb's mean corrections as the proper corrections to be applied to the tabular places resulting from the theory described by him and have tabulated them in column *a* of Table II. It is not likely that any means obtained by another method would be superior to those given by him. For the group with mean epoch 1681 I have substituted the exact value −14″·0 for the vaguer −14″, as a combination of the value given on p. 127 with the reduction given on p. 203.

Columns *c* and *d* in Table II. have the same meaning as the columns with the same headings in Table I.

The only sensible difference between Newcomb's and Brown's terms of short and mean period is in the case of the term depending on the longitude of the Moon's node. In *M.N.*, 73, p. 702, Dr. Brown gives as the correction to be applied to Newcomb's longitude in respect of this term

$$-1''\cdot075 \sin \Omega + 0''\cdot060 \cos \Omega,$$

where Ω is the longitude of the Moon's ascending node. This reduces to the monomial

$$+1''\cdot08 \sin (3^\circ - \Omega).$$

In order that each correction should have the same effect that it would have had if it had been introduced by Newcomb in

computing the place of the Moon, I have applied this correction to the longitudes resulting from each separate occultation used by Newcomb down to 1747 and to the occultations which I believe to have been used by him for the years 1753, 1754, 1757. In consequence in each of the five groups on pp. 121–122 of the second part of the *Researches*, and again for the observations of 1753 Apr. 19, the first and second of 1753 Oct. 5, and those of 1754 Apr. 2, Nov. 21, 1757 July 30, I have multiplied the correction in each case by the square of the coefficient of λ for the particular observation and by the weight attached to the particular observation. The sum of these products has been divided by the sum of the coefficients of λ multiplied by their respective weights, and the quotient with its sign changed has been adopted as the final correction in respect of this term for the particular group. The correction for the epochs given on p. 206 of the second part of the *Researches* and in my Table II. was then found by interpolation. In the case of Group IV., mean epoch 1738, the preponderance of observations of Aldebaran seemed to render it desirable to obtain the best place for this star, and Boss's longitude was, therefore, substituted or Newcomb's as corrected for magnitude equation. Elsewhere it was assumed that the mean error in the star places was negligible.

After 1757 it seemed impossible to determine which observations Newcomb had used. For this reason and because of the increasing frequency of the observations it seemed best to assign to the correction in respect of the term depending on the Moon's node its mean value for the interval covered by each of the small groups on p. 193, down to the group with mean epoch 1821·3. For this purpose means were taken of the mean epochs of groups of immersions and emersions, and the most probable limits were selected for the occultations included within these groups. There were generally found to be distinct gaps between the different groups. The value of the correction was taken as

$$-1''\cdot08\,\frac{\sin v}{v}\sin(3^\circ-\Omega),$$

where v is half the change in the value of Ω during the interval represented by each group, and the value of Ω is computed for the mean epoch of the group. The value for each epoch used in the table was then found by interpolation.

Epochs 1830 onwards were assumed to represent ten-year averages, and the mean correction at each epoch was taken to be $-0''\cdot63\sin(3^\circ-\Omega)$.

n is the sum of a, c, d, and f, and represents the total correction to be applied to Dr. Brown's tabular places when freed from the empirical term.

In assigning to each group weight unity I am following the example of Newcomb, who writes as follows:—"From the commencement of fairly precise observations in 1672 until the present time the purely accidental errors of observation are markedly

less than the outstanding fluctuations [after the application of corrections to the secular terms and the introduction of an empirical term of long period], which are systematic in their character." The residuals obtained by Newcomb seem to show that this is hardly true for any mean epoch before 1780. But the reason for this lies as much in the smallness of the outstanding fluctuations before that date as in any inferiority of the older to the more modern observations, and it appears to me that substantial justice is done by treating the probable error of each mean correction as $\pm 1''$. The more modern observations are then given this much advantage that by the use of regular ten-year means as opposed to more scattered means they have, so long as all groups are weighted equally, a rather greater combined effect.

TABLE II.

Epoch.	*a*.	*c*.	*d*.	*f*.	*n*.	Weight.
	$''$	$''$	$''$	$''$	$''$	
1681	−14·0	+2·79	−0·07	+0·49	−10·8	1
1710	− 5·2	+0·93	−0·13	−0·10	− 4·5	1
1727	− 0·1	+0·03	−0·22	−0·37	− 0·7	1
1737	+ 2·8	−0·43	−0·27	+0·32	+ 2·4	1
1747	+ 6·0	−0·85	−0·36	−0·31	+ 4·5	1
1757	+ 8·3	−1·22	−0·44	0	+ 6·6	1
1780	+13·9	−1·88	−0·66	−0·36	+11·0	1
1795	+13·3	−2·17	−0·80	−0·22	+10·1	1
1806	+12·4	−2·32	−0·89	−0·11	+ 9·1	1
1820	+11·5	−2·42	−0·98	−0·23	+ 7·9	1
1830	+ 8·3	−2·44	−1·05	+0·11	+ 4·9	1
1840	+ 7·6	−2·40	−1·08	−0·25	+ 3·9	1
1850	+ 6·3	−2·32	−1·10	+0·38	+ 3·3	1
1860	+ 6·2	−2·20	−1·10	−0·48	+ 2·4	1
1870	− 0·3	−2·01	−1·09	+0·56	− 2·8	1
1880	− 4·1	−1·79	−1·08	−0·61	− 7·6	1
1890	− 8·5	−1·51	−1·05	+0·63	−10·4	1
1900	− 7·6	−1·19	−1·00	−0·61	−10·4	1

1900 is the epoch of the last of Newcomb's ten-year means. He closes his series with an equation for 1908 which appears to be a mean based on the observations of 1907 and of the first eight months of 1908. I have preferred to construct an equation for 1910 representing the mean of ten years from 1905 to 1914 inclusive, and an equation for 1917 representing the mean of the four years from 1915 to 1918 inclusive.

For the years 1905, 1906, 1907 I use the corrections obtained by Newcomb on p. 194 of the second part of his *Researches* as applicable to his provisional theory, and apply the proper correction for the term dependent on the longitude of the node. I thus obtain

TABLE III.

Epoch.	Newcomb's Correction.	*f*.	Combined Correction.
1905·5	+5″·62	+0″·55	+6″·17
1906·5	+5·34	+0·82	+6·16
1907·5	+6·11	+1·01	+7·12

For 1908 to 1918 I use the Greenwich transit and altazimuth observations. These have not been reduced with the same rigour with which Newcomb has reduced the occultations. The actual error in longitude is deduced from the observed Right Ascension and declination for each observation, and the arithmetical mean of the individual errors is the only annual mean computed. The individual errors are affected by the anomalistic motion of the Moon, and an annual mean from which this is not eliminated is for all practical purposes less accurate than one found after eliminating this inequality. The mean obtained by the Greenwich method could only be considered accurate if the observations were well distributed over the Moon's orbit. If they were so distributed the ratio between the mean error in right ascension and the mean error in longitude would have a constant value for any given position of the node and would, therefore, recur at intervals of 19 years. A comparison of the figures for 1899 with those for 1918 shows that this is not necessarily the case. For 1899 the report of the Astronomer Royal shows an error in R.A. (tabular − observed) of −0ˢ·099 and in longitude of −1″·38, a ratio of 13″·9 of longitude to 1ˢ of R.A., whereas for 1918 the figures are −0ˢ·92 in R.A. and −14″·05 in longitude, a ratio of 15″·3 of longitude to 1ˢ of R.A. In order to eliminate the effect of the anomalistic motion of the Moon each individual correction in longitude should be multiplied by $\frac{1}{1+0\cdot11\cos g}$, *i.e.* by $1\cdot006 - 0\cdot11\cos g + 0\cdot006\cos 2g$, where g is the mean anomaly of the Moon. Assuming that the observations are distributed at random over the Moon's elliptical orbit and that, as in 1918, observations were made on 125 nights in the year. the mean error in the Greenwich mean correction for the year will be $+0\cdot006\epsilon \pm \frac{0\cdot11\epsilon}{\sqrt{125}} \cdot \frac{2}{\pi}$, *i.e.* $+0\cdot006\epsilon \pm 0\cdot006\epsilon$, where ϵ is the mean correction for the year. Where the annual mean correction has its present value of +14″, this expression assumes the value of +0″·08 ± 0″·08. I retain the constant part of this correction and multiply each Greenwich mean correction by 1·006. The variable part cannot appreciably affect a ten-years' mean, and is, therefore, unimportant for my present purpose.

As the Greenwich corrections are applied to Hansen's places modified only by the introduction of Newcomb's values for the secular terms and the substitution of his empirical term of long period for the larger of Hansen's Venus inequalities, it is necessary to correct each year's correction in order to reduce it to Dr. Brown's values for the theoretical inequalities of mean and short period. Dr. Brown gives the necessary corrections in *M.N.*, 73, pp. 696, 698. I have excluded those corrections which could not possibly affect an annual mean by as much as 0″·07. The remaining corrections are:—

(1) $+0''\cdot284 \sin (3J - 2T + 2l - 2D + 172°\cdot5)$.
(2) $+0\cdot282 \sin (264°\cdot0 - \Omega)$.
(3) $+0\cdot240 \sin (2T - 2J - 2l + 2D + 0°\cdot1)$.
(4) $+0\cdot126 \sin (20V - 21T + l' - 2D + 267°)$.
(5) $+0\cdot108 \sin (26V - 29T - l' + 68°)$.
(6) $+0\cdot075 \sin (2D - l' + T - 3Q + 105°)$.
(7) $+0\cdot073 \sin (3V - 3T - 2l + 2D)$.
(8) $+0\cdot49 \sin (l - l' - D)$.
(9) $-0\cdot28 \sin (T - V)$.
(10) $+0\cdot35 \sin (3T - 2V + 273°)$.
(11) $+0\cdot09 \sin (5T - 3V + 199°)$.
(12) $+0\cdot09 \sin (J + 290°)$.
(13) $-1\cdot22 \sin (\Omega + 325°)$.

In the last of these terms I have corrected an error of sign which occurs both on p. 698 and on p. 699 of Dr. Brown's paper. The correct value appears to have been used in his actual reductions. I have also taken account of the correction—

(14) $-0''\cdot60 \sin (l - D)$.

The coefficients of terms (9), (10), (14) have been reduced to $-0''\cdot131$, $+0''\cdot315$, $-0''\cdot075$ respectively in accordance with the principle adopted above in the case of the term dependent on the longitude of the node. All the terms have been computed for the middle of each year. Table IV. gives under the heading *a* the mean correction for the year as taken from the Astronomer Royal's report (1919), but with the sign altered so as to make it observed—tabular. Under *b* I tabulate the same quantity multiplied by 0·006. *c* is the sum of the fourteen corrections enumerated above with the sign altered, so as to make them additive to the original correction. *h* is the sum of *a*, *b*, and *c*, and represents the correction to the *Nautical Almanac* places corrected to Dr. Brown's values for the terms of short and mean period.

TABLE IV.

Epoch.	*a*	*b*	*c*	*h*
1908·5	+5″·97	+0″·04	+1″·28	+7″·29
1909·5	+6·41	+0·04	+0·82	+7·27
1910·5	+7·85	+0·05	+0·46	+8·36
1911·5	+8·34	+0·05	+0·50	+8·89
1912·5	+9·79	+0·06	−0·47	+9·38
1913·5	+11·93	+0·07	−1·49	+10·51
1914·5	+12·86	+0·08	−1·38	+11·56
1915·5	+12·58	+0·08	−1·28	+11·38
1916·5	+14·05	+0·08	−1·20	+12·93
1917·5	+14·03	+0·08	−1·18	+12·93
1918·5	+14·05	+0·08	−0·86	+13·27

Combining the "combined correction" shown in Table III. for the epochs 1905·5 to 1907·5 with the values of *h* for epochs 1908·5 to 1914·5, I find a mean correction for epoch 1910, while from the values of *h* for epochs 1915·5 to 1918·5 I find a mean correction for epoch 1917. These are tabulated as *a* in Table V. *b* is the reduction to what Newcomb calls "pure theory" and is, for epoch 1910, taken from the table on p. 203 of the second part of the *Researches*, while for 1917 it is computed from the formula given on p. 202 of that work. *c* and *d* have the same meanings as in Tables I. and II. *n* is the sum of *a*, *b*, *c*, and *d*, and represents the observed correction to Dr. Brown's places freed from his empirical term of long period. The equation for mean epoch 1917 receives a weight of $\frac{2}{5}$, because it represents four years' observations instead of ten.

TABLE V.

Epoch.	*a*	*b*	*c*	*d*	*n*	Weight.
1910	+8″·27	−12″·80	−0″·82	−0″·96	−6″·3	1
1917	+12·63	−13·78	−0·53	−0·92	−2·6	$\frac{2}{5}$

With the values of *n* given in Tables I., II., V., I have formed six series of equations of condition, based on the assumptions (1) that no empirical periodic term is to be introduced, and (2) to (6) that such a term is to be introduced, the annual motion of the argument being taken as 1°·20, 1°·30, 1°·40, 1°·50, 1°·60 in succession. Each equation is made to assume the form

$$x + Ty + T^2z + u \sin Bt + v \cos Bt = n,$$

where x is the correction to Dr. Brown's mean longitude at epoch 1810·0, y is the correction to his centennial motion, and z to his acceleration at the same epoch. T is reckoned in Julian centuries, and t in Julian years from 1810·0; B is the assumed annual motion of the argument of the empirical term, u is the coefficient of that term multiplied by the cosine of its argument at epoch 1810·0, and v is the coefficient multiplied by the sine of the argument at the same epoch. The two first epochs of Table I. were rounded off to 1636 and 1645.

Giving each equation the weight assigned to it in Tables I., II., V., I have found the following results by least squares solutions :—

TABLE VI.

	I.	II.	III.	IV.	V.	VI.
Assumed annual motion of argument	...	1°·20	1°·30	1°·40	1°·50	1°·60
Length of period in years	...	300	277	257	240	225
$x=$	+ 7″·18	−10″·43	− 5″·52	− 2″·05	+ 0″·33	+2″·12
$y=$	− 4·44	+ 6·83	+ 4·66	+ 2·87	+ 1·55	+0·40
$z=$	−13·89	+13·98	+ 8·78	+ 4·52	+ 1·27	−1·50
$u=$	...	− 9·17	− 7·57	− 6·33	− 5·48	−4·79
$v=$	...	+20·13	−15·23	+11·78	+ 9·42	−7·65
Coefficient of empirical term	...	22·12	17·01	13·37	10·90	9·02
Argument of empirical term in 1810·0	...	114°·5	116°·4	118°·2	120°·2	122°·0
Sum of weighted squares of residuals	188″·1	55″·9	57″·5	59″·3	61″·5	63″·9

Table VII. exhibits the residuals (observed − tabular) at each mean epoch as derived from each of the six solutions, and also those resulting from Dr. Brown's theory including his empirical term.

TABLE VII.

Residuals.

Epoch.	I.	II.	III.	IV.	V.	VI.	Brown.
	″	″	″	″	″	″	″
1636	+34	+ 9	+10	+11	+12	+14	+15
1645	+ 6·6	−11·1	−10·5	− 9·9	− 9·3	− 8·5	− 7·1
1662	+ 7·4	+ 0·4	+ 0·4	+ 0·5	+ 0·6	+ 0·7	+ 1·4
1681	− 0·6	− 0·5	− 0·6	− 0·7	− 0·8	− 0·9	− 1·0
1710	− 2·2	+ 0·9	+ 0·9	+ 0·9	+ 0·9	+ 0·8	+ 0·1
1727	− 2·0	+ 0·1	+ 0·2	+ 0·2	+ 0·3	+ 0·3	− 0·4
1737	− 0·6	+ 0·4	+ 0·4	+ 0·5	+ 0·5	+ 0·6	+ 0·1
1747	0	− 0·2	− 0·2	− 0·2	− 0·1	− 0·1	− 0·3
1757	+ 1·0	− 0·4	− 0·4	− 0·4	− 0·3	− 0·3	− 0·3
1780	+ 3·7	+ 0·5	+ 0·5	+ 0·5	+ 0·5	+ 0·5	+ 0·8
1795	+ 2·6	− 0·7	− 0·8	− 0·8	− 0·8	− 0·9	− 0·6
1806	+ 1·7	− 1·1	− 1·1	− 1·1	− 1·1	− 1·2	− 1·0
1820	+ 1·3	− 0·3	− 0·3	− 0·3	− 0·3	− 0·3	− 0·5
1830	− 0·8	− 1·2	− 1·2	− 1·2	− 1·2	− 1·2	− 1·6
1840	− 0·7	+ 0·1	+ 0·1	+ 0·1	+ 0·2	+ 0·2	− 0·4
1850	+ 0·1	+ 2·1	+ 2·1	+ 2·1	+ 2·1	+ 2·2	+ 1·5
1860	+ 0·9	+ 3·8	+ 3·8	+ 3·8	+ 3·8	+ 3·9	+ 3·3
1870	− 2·4	+ 1·0	+ 1·0	+ 0·9	+ 0·9	+ 0·9	+ 0·6
1880	− 4·8	− 1·8	− 1·8	− 1·8	− 1·9	− 1·9	− 1·8
1890	− 5·2	− 3·2	− 3·2	− 3·3	− 3·4	− 3·4	− 2·6
1900	− 2·3	− 2·5	− 2·5	− 2·6	− 2·6	− 2·7	− 1·1
1910	+ 4·8	+ 1·3	+ 1·4	+ 1·4	+ 1·4	+ 1·5	+ 4·0
1917	+10·9	+ 4·2	+ 4·4	+ 4·6	+ 4·7	+ 4·9	+ 8·1

It will be observed that while Solution I. gives a distinctly inferior result, the five solutions which assume an empirical term yield practically identical results, and the choice between them cannot be determined by the modern observations. The acceleration, however, can be determined from ancient observations, and the best value appears to be in the neighbourhood of $+10''$ per century. As Dr. Brown uses $+6''\cdot01$, it will be seen that Solution IV. gives a value of $+10''\cdot53$, while Solution V. gives $+7''\cdot28$. This suggests that 254 years is perhaps the best length for the period of the empirical term. But, as the secular acceleration has not been very accurately determined, nothing is gained by substituting a new solution for Solution IV., which has the advantage of retaining Dr. Brown's value for the period of the empirical term. The correction to Dr. Brown's mean longitude, as given by this solution and reduced to epoch 1800·0, is:—

$$-2''\cdot29 + 1''\cdot96T + 4''\cdot52T^2.$$

The empirical term, reduced to the same epoch, is

$$+13''\cdot37 \sin(104^\circ\cdot2 + 140^\circ T),$$

where T is reckoned in Julian centuries.

Dr. Brown's value is

$$+10''\cdot71 \sin(100^\circ\cdot7 + 140^\circ T).$$

The correction to Dr. Brown's value of this term is, therefore:—

$$+2''\cdot76 \sin(118^\circ\cdot1 + 140^\circ T).$$

The combined correction to Dr. Brown's mean longitude and terms of long period becomes:—

$$-2''\cdot29 + 1''\cdot96T + 4''\cdot52T^2 + 2''\cdot76 \sin(118^\circ\cdot1 + 140^\circ T),$$

and the correction to his value for the true longitude will be found by multiplying this expression by

$$1 + 0\cdot110 \cos g + 0\cdot023 \cos 2D + 0\cdot022 \cos(2D - g) + 0\cdot007 \cos 2g,$$

where g represents the mean anomaly of the Moon, and D the mean elongation of the Moon from the Sun.

The adoption of this correction should substantially satisfy both the ancient and the modern observations.

I have not attempted to compute the probable error of each of the terms for which values have been found in this paper, and any attempt to determine such errors must be received with caution, since the theory assumes that the errors are fortuitous, whereas in the present case they are clearly systematic. I have, however, determined for what it may be worth the probable error attaching to the values of the acceleration on each of the three assumptions that the annual motion of the argument of the empirical term is $1^\circ\cdot40$, $1^\circ\cdot50$, and $1^\circ\cdot60$. The corresponding prob-

able errors are found to be $\pm 2''\cdot3$, $\pm 1''\cdot9$, and $\pm 1''\cdot6$. Professor Turner has, in *M.N.*, 79 (1919), pp. 531–539, deduced from the records of Chinese earthquakes and of Nile floods a period of about 240 years, and has suggested that it may be identical with the period in the Moon's motion investigated in this paper. It will be seen from Table VI. that a period of 240 years requires an addition of $+1''\cdot55$ to Dr. Brown's value of the acceleration, making the total acceleration $+7''\cdot56$. The corresponding probable error has just been found to be $\pm 1''\cdot9$. So that, if $+10''$ is the best value for the acceleration, a 240 years' term could be reconciled with it on the assumption that the error in the acceleration deduced in this paper is rather more than the probable error. If for 240 years we substitute 243 years, as Professor Turner tentatively suggests, the present inquiry gives a corresponding acceleration of $+7''\cdot84 \pm 2''\cdot0$. In any case the present inquiry does not, like previous inquiries, leave an improbable discrepancy between the period determined from terrestrial phenomena and the period determined from lunar observations, such as might render it difficult to believe that the same period ran through both classes of phenomena.

Applying the correction obtained to Dr. Brown's mean longitude, I obtain for the final mean longitude deduced in this paper

$$335^\circ 43' 25''\cdot22 + 1336^r\ 307^\circ 53' 13''\cdot76\mathrm{T} + 11''\cdot64\mathrm{T}^2 + 0''\cdot0068\mathrm{T}^3,$$

where T is the interval in Julian centuries since 1800 Jan. 0·0 G.M.T.

The most striking result of this paper is not that it obtains widely different results from those previously obtained for the constants of the secular terms and large empirical term, for ever since the appearance of Dr. Cowell's paper in 1904 it has been recognised that there is no unique solution, but that the present paper associates particular values for one constant with quite other values for the remaining constants than those with which they would be associated by a mere transformation of Newcomb's, Radau's, or Brown's formulæ. Thus with the same value for the period of the empirical term I find an acceleration exceeding Dr. Brown's by $+4''\cdot52$. This result is due almost entirely to the inclusion of the observations of the last ten years and not to the exclusion of the solar eclipses, nor to the correction of Newcomb's erroneous coefficient of the term dependent on the node. In order to test this I have made a separate solution, assuming as above that the annual motion of the argument of the empirical term is $1^\circ\cdot40$, and substituting for the equations for mean epochs 1910 and 1917 an equation for epoch 1908 based on the places of the Moon in 1907 and 1908, and I have, like Newcomb and Brown, assigned full weight to this equation. This solution gives for the secular acceleration $+6''\cdot80 \pm 2''\cdot15$, differing from Dr. Brown's value by about one-sixth as much as my final solution, which took account of the observations down to 1918.

In order to understand how an additional ten years of observation could make so great a difference to the deduced constants it is necessary to study the residuals which I have set out in Table VII. It will be seen that the groups with mean epochs 1662 to 1795 show residuals of less than 1″. The later residuals assume far larger dimensions, and it is easy to trace through them a period of sixty or seventy years, signs of which appear even among the earlier and smaller residuals. The residuals taken as a whole show a minor fluctuation with an increasing amplitude, and, if they are to be represented by a trigonometric series it will be necessary either to use a varying coefficient or to assume the interference of at least two terms which tend to have opposite phases during the eighteenth century and identical phases at the present day. At all events the minor fluctuation has not completed much more than one period since its amplitude became pronounced, and we can hardly expect the residuals which it produces to be at all evenly distributed between positives and negatives in so short an interval. A least squares solution is, therefore, dragged this way and that by a desire to keep down the concluding residuals of the series. This is well illustrated by Table VIII., in which I compare the weighted residuals yielded by Newcomb's solution (in his own form uncorrected for the nodal inequality) with those obtained by my Solution IV.

TABLE VIII.

Residuals.

Epoch.	Newcomb.	Solution IV.
1850	+1″·2	+2″·1
1860	+3·8	+3·8
1870	0	+0·9
1880	−1·2	−1·8
1890	−3·2	−3·3
1900	−0·4	−2·6
1908	+2·1	(+0·7)
1910	(+4·0, Brown, with nodal correction)	+1·4
1917	(+8·1, Brown, with nodal correction)	+4·6
Sum of squares of residuals, 1850 to 1908.	32·1	41·1 (1850 to 1908) 42·6 (1850 to 1910, omitting 1908).

It will be observed that Newcomb's series ends immediately after the close of a chapter of negative residuals. A least squares solution with five unknowns is able greatly to reduce these negative residuals without greatly modifying individual previous residuals. The result is that it is bound to apply a false and increasing negative correction to the Moon's true place, producing

a modification of the residuals in a positive sense. In fact it would have been difficult to select a date for the close of the series which would have been more certain to produce an erroneous result than the date at which Newcomb happened to close it. Ten more years of observations carry the series well into a positive chapter, and the false negative corrections, combined with the residuals arising from the real minor fluctuation, assume a dimension which the method of least squares will not tolerate. The application of that method down to 1918 tends, therefore, to correct the error obtained by ending at the close of the negative chapter. It is difficult to say how far the amplitudes of $+4''{\cdot}6$, which I found for the mean of the four years 1915 to 1918 inclusive, and of $+5''{\cdot}0$, which I have found for 1916, regarded separately indicate a real increase in the amplitude of the minor fluctuation, and how far they are to be explained by the supposition that the distortion of the result by the last negative chapter is not yet exhausted. On the latter assumption the later lunar places obtained in this paper still require a positive correction, from which it would follow that the acceleration here found to cohere with a given period of the empirical term must be slightly increased, and, if the absolute value of the acceleration does not admit of increase, the period must be shortened. This confirms Professor Turner's suggestion that the period which he has discovered is really identical with the period of the large empirical term.

It would be advantageous to eliminate the minor fluctuation as far as possible before performing the final solution for the large empirical term, but this elimination is not easily accomplished. The existing determinations of the minor fluctuation do not explain its increasing amplitude, and most of them presuppose a particular value for the combined effect of the secular terms and large empirical term. As the minor fluctuation has been found in the Sun's longitude as well as the Moon's,* it is, perhaps, best made part of a wider inquiry, which cannot be attempted in this paper.

6 *Blackhall Road, Oxford:*
1919 *December* 31.

* See a paper by Dr. Brown read before the British Association in 1914, pp. 319–21, papers by Mr. Glauert in *M.N.*, 75 (1915), pp. 489–95, 685–7, and, most important of all, Dr. Ross's paper in the *Astronomical Journal*, 29 (1916), pp. 152–6.

Tidal Friction and the Secular Acceleration of the Moon.

By G. I. Taylor, M.A., F.R.S.

(*Communicated by Harold Jeffreys.*)

In a recent paper on "Tidal Friction in the Irish Sea," * I have calculated the mean rate of dissipation of energy by tidal currents in the Irish Sea at spring tides. Two separate methods are used:—

(1) Reasons are given for believing that the formula $F = \cdot 002\rho V^2$ obtained by the author for the friction, F, of the wind on the ground is applicable to tidal currents. In this formula ρ represents the density of the sea and V the velocity of the current.

A similar expression is obtained from the formulæ used by engineers connecting the slope of a river with the velocity of the current.

This expression makes the rate of dissipation of energy 1300 ergs † per sq. cm. per second.

(2) The rate at which energy enters the Irish Sea through the north and south channels is calculated. From this is subtracted the rate at which lunar attraction does work on the waters of the Irish Sea. The result is equivalent to a dissipation of 1530 ergs ‡ per sq. cm. per second.

These figures apply to the rate of dissipation of energy at spring tides. In dealing with the secular accelerations of the Sun and Moon, Dr. Jeffreys calculates the mean rate of absorption of tidal energy which would be necessary in order to account for the secular accelerations which appear to be observed. To reduce the dissipation at spring tides to the mean dissipation, it is sufficient to assume that the tidal currents vary during the month in accordance with an expression of the type

$$V = \frac{V_s + V_n}{2} + \frac{V_s - V_n}{2} \cos nt,$$

where t represents time and V_s and V_n are the velocities of the tidal currents at springs and neaps respectively.

In the case of the Irish Sea the ratio of V_s to V_n is about 2:1. It is slightly less than this in places where the currents run strongly. Taking therefore $V_n = \frac{V_s}{2}$, $\frac{V}{V_s} = \frac{3 + \cos nt}{4}$ and remembering that the rate of dissipation of energy is proportional to V^3, it will be seen that the mean rate of dissipation of energy must be less than that at spring tides in the ratio of the mean value of $(3 + \cos nt)^3$ to 4^3. This ratio is equal to $\frac{31\frac{1}{2}}{64}$, or $\frac{1}{2}$ nearly.

The area of the Irish Sea is $3 \cdot 9 \times 10^{14}$ sq. cm. Hence the mean rate of dissipation of energy in the Irish Sea is

$$3 \cdot 9 \times 10^{14} \times 1300 \times \tfrac{1}{2} = 2 \cdot 5 \times 10^{17} \text{ ergs per second,}$$

* *Phil. Trans.*, A, 220. † *Ibid.*, p. 9. ‡ *Ibid.*, p. 21.

according to our first estimate; or

$$3{\cdot}9 \times 10^{14} \times 1530 \times \tfrac{1}{2} = 3{\cdot}0 \times 10^{17} \text{ ergs per second,}$$

according to our second estimate.

The rate of dissipation of energy necessary for producing a secular acceleration of the Moon of 9 seconds per century per century is shown by Dr. Jeffreys in the following paper to be $1{\cdot}41 \times 10^{19}$ ergs per second.

According to these calculations, therefore, the Irish Sea would contribute $\frac{2{\cdot}5 \times 10^{17}}{1{\cdot}4 \times 10^{19}}$, or $\frac{1}{56}$th part of the total dissipation.

When it is remembered that the Irish Sea covers only $\frac{1}{10000}$th of the water area of the globe, it would appear, at first sight, that the Irish Sea contributes a very much greater proportion of the necessary dissipation than it ought. It must be remembered, however, that the rate of dissipation of energy is proportional to the cube of the tidal current. The open ocean and all places where the tidal currents are very small may therefore be expected to contribute practically nothing to the total dissipation. When one comes to examine the places on the earth's surface where the tidal currents are large, it will almost always be found that they are confined to small areas close to the shore.

The most favourable basins for tidal dissipation are long bays and channels where the water is comparatively shallow. Further, these must be situated in places where the general ocean tides tend to produce a large rise and fall of tide. The Irish Sea appears to be a specially favourable basin, so does the English Channel; but the North Sea is not so favourable, the tidal currents being chiefly confined to the coasts.

The conclusion to be drawn from these results is that the dissipation of energy in the tides is amply sufficient, perhaps more than sufficient, to account for a secular acceleration of the Moon amounting to 9 seconds per century per century, since only 56 basins, as favourably situated as the Irish Sea for tidal dissipation, are needed.

The Chief Cause of the Lunar Secular Acceleration.

By Harold Jeffreys, M.A., D.Sc.

It has long been known that the records of ancient eclipses indicate that the moon has relative to the sun an apparent acceleration of its mean motion of about 22″ per century per century.* Various explanations of this have been offered, but hitherto none have been found wholly satisfactory. Laplace's lunar theory indicated a secular acceleration of about this amount, but was shown to be in error by Adams, the amount explicable by purely gravitational

* Equivalent to 11″ in a century, in the language of most writers on the subject.

causes being proved by him to be 12″·2 per century per century. The balance has generally been held to be due to tidal friction, though it has never been shown that in any part of the earth sufficient tidal friction actually takes place.

In the body of the earth tidal friction can arise only through imperfection of elasticity, and it seems that this can always be regarded as a combination of two main types, namely, plasticity and elastic afterworking. In the former of these the imperfection is due to incompleteness of the elastic recovery after strain, and in the latter to slowness of the recovery, even though it may in time become complete. The plastic type may be typified by the permanent distortions readily produced in copper and pitch; the afterworking by the gradual creeping back to the original state observed in cast iron and sealing-wax when these have been subjected to small strains. Now, few things in geophysics are more certain than that the outer two thousand miles or so of the crust must be practically free from both these qualities when small stresses alone are considered. Plasticity sufficient to account for the secular acceleration would damp out the 14-monthly variation of latitude in a few days,* whereas it actually persists for several years; while afterworking would render the crust perfectly impermeable to earthquake waves.† Nearer the centre both phenomena may be more important, but there is no evidence of an increase great enough to account for the secular acceleration. Knott's recent discovery ‡ of a region near the centre of the earth impenetrable to earthquake waves offers no solution of the present problem, for if the extinction he notices is due to elastic afterworking, his result shows only that an earthquake wave is extinguished in traversing about 3000 km., which does not imply enough imperfection of elasticity to account for any perceptible secular acceleration. Thus while his results would be consistent with the explanation of the secular acceleration by means of afterworking near the centre of the earth, they afford no reason to believe that a sufficient amount actually exists there. The explanation by means of plasticity, which Knott himself appears to favour, is more difficult to investigate. Such plasticity when the strains have periods of a few seconds would mean that the affected region would behave as a perfect fluid to both the semidiurnal bodily tide and the 14-monthly variation of latitude; it would therefore imply a reduction of effective rigidity in these movements, without necessarily any considerable dissipation of energy. It might even be possible to have a notable dissipation of energy in the semidiurnal tide and not in the variation of latitude. The dissipation would then have to occur in the region where Knott finds that waves are able to travel; for the time of relaxation must be of the order of a day to give appreciable dissipation, and such a substance could produce no appreciable effect on a seismic wave in the time it takes to pass through the earth. This time

* *Monthly Notices*, 75, 648–658, 1915. † *Ibid.*, 77, 449–456, 1917.
‡ *Proc. Roy. Soc. Edin.*, 39, 188–208, 1919.

of relaxation would, however, imply that the region concerned behaves as a perfect fluid to the 14-monthly motion; thus in this the earth would behave as a perfectly elastic shell of considerable thickness, enclosing a perfect fluid. Such a system offers no opportunity for dissipation of energy, and the variation of latitude could therefore persist. With suitable assumptions the data could therefore probably be reconciled with the hypothesis of local plasticity; though the method seems somewhat artificial. The prior probability that the impenetrability and the tidal dissipation are due to plasticity is difficult to estimate. If the earth was formerly fluid, the centre must be the part that has changed least since solidification, and it is there, if anywhere, that plasticity may be expected. Any agreement between theory and observation must, however, be based on purely *ad hoc* quantitative assumptions, and an alternative explanation that will account for the secular acceleration by known causes is preferable, if such can be obtained.

Atmospheric phenomena are incapable of accounting for the secular acceleration. The gravitational tide is insignificant, but the thermal effects cause a semidiurnal variation of the barometer, capable of causing a small effect in the opposite direction to that observed.

Tides in mid-ocean have been discussed several times, and it appears certain that neither viscosity nor turbulence there can cause sufficient friction to account for the secular acceleration. The first true cause found capable of producing an effect of the correct order of magnitude is that considered by G. I. Taylor in his recent paper on tidal friction in the Irish Sea.* In this partially land-locked area the tidal currents are so much enhanced by the form of the sea-bottom that the skin friction on the bottom causes a dissipation of energy greater than that over the whole of the deep ocean, and it is here proved capable of accounting for about $\frac{1}{50}$ of the secular acceleration. If, then, there are 50 Irish Seas in the world the problem may be regarded as solved; but the counting of them is difficult. Large areas probably contribute little to the dissipation, since the currents are small and the dissipation depends on the cube of the velocity. Small areas again may contribute little, on account of their smallness itself. The important regions appear to be partly enclosed seas of moderate size. Such are the English Channel, the Bay of Fundy, Behring Strait, the Mozambique Channel, and probably several areas in the Sea of Japan and the East and West Indies; these seem to exhaust the list of likely places. The Mediterranean and the Baltic contribute next to nothing on account of the smallness of their currents. On the whole, therefore, allowing for the greatness of the tidal currents in some of these regions, it seems quite probable that they are capable of causing the whole of the secular acceleration.

Certain further consequences of this theory are deduced; the theoretical secular acceleration of the sun is determined, and it is

* *Phil. Trans.*, **220**, 1–33, 1919.

shown that the effect of such tidal friction on the variation of latitude is insignificant. It obviously can have no effect on earthquake waves. The theory accordingly appears to be free from objection.

Tidal Friction and the Lunar and Solar Secular Acceleration.

Let the masses of the earth, moon, and sun be respectively M, m, and m_1.

Let the angular velocities of the moon and sun about the earth be n and n_1, and their distances from the earth c and c_1.

Let the frictional couple due to the lunar tides retarding the earth's rotation be N, and that due to the solar tides be N_1.

Let the earth's angular velocity of rotation be Ω, and its principal moment of inertia C.

Then

$$n^2c^3 = f(M+m)\,;\ n_1^2c_1^3 = f(m_1+M) \quad . \quad . \quad (1)$$

where f is the constant of gravitation.

Put

$$c = c_0\xi^2,\ n = n_0\xi^{-3}\,;\ c_1 = c_{10}\xi^2,\ n_1 = n_{10}\xi_1^{-3}\,. \quad . \quad (2)$$

where c_0 and n_0 are the present values of c and n.

The angular momentum of the moon's orbital motion is $\frac{Mm}{M+m}c^2n$; that of the earth's orbital motion is $\frac{m_1M}{m_1+M}c_1^2n_1$; and that of the earth's rotation is $C\Omega$. The equations giving the true secular accelerations are therefore:

$$\frac{Mm}{M+m}c_0^2n_0\frac{d\xi}{dt} = N \quad . \quad . \quad . \quad . \quad (3)$$

$$\frac{m_1M}{m_1+M}c_{10}^2n_{10}\frac{d\xi_1}{dt} = N_1 \quad . \quad . \quad . \quad (4)$$

$$C\frac{d\Omega}{dt} = -N - N_1 \quad . \quad . \quad . \quad . \quad (5)$$

The effect of the variation in the speed of rotation of the earth is to make the transit of a fixed star occur sooner than it otherwise would, by a time $\frac{T^2}{2\Omega}\frac{d\Omega}{dt}$, where T is the time-interval elapsed from the first estimation of Ω. The moon moving relative to the stars with mean motion n is, therefore, an arc $\frac{nT^2}{2\Omega}\frac{d\Omega}{dt}$ behind its calculated position when the star transits; its own secular acceleration, however, puts it ahead an amount $\frac{1}{2}T^2\frac{dn}{dt}$; hence the observed excess motion in time T of the moon relative to the

stars is the difference of these quantities. Thus the moon appears to have a steady acceleration

$$\nu = \frac{dn}{dt} - \frac{n}{\Omega}\frac{d\Omega}{dt} = -3n_0\xi^{-4}\frac{d\xi}{dt} - \frac{n_0}{\Omega}\xi^{-3}\frac{d\Omega}{dt} \quad . \quad . \quad (6)$$

Similarly the apparent secular acceleration of the sun is

$$\nu_1 = \frac{dn_1}{dt} - \frac{n_1}{\Omega}\frac{d\Omega}{dt} = -3n_{10}\xi_1^{-4}\frac{d\xi_1}{dt} - \frac{n_{10}}{\Omega}\xi_1^{-3}\frac{d\Omega}{dt} \quad . \quad (7)$$

Substituting for the differential coefficients in terms of N and N_1, we have

$$\nu = -3\frac{M+m}{Mm}\frac{N\xi^{-4}}{{c_0}^2} + \frac{N+N_1}{C\Omega}n_0\xi^{-3} \quad . \quad . \quad (8)$$

$$\nu_1 = -3\frac{m_1+M}{m_1 M}\frac{N_1\xi_1^{-4}}{{c_{10}}^2} + \frac{N+N_1}{C\Omega}n_{10}\xi_1^{-3} \quad . \quad . \quad (9)$$

So far no hypothesis has been made, except that the mutual actions are couples in the planes of the orbits. Suppose now that they arise from tidal friction. Then as N and N_1 are of the same order of magnitude, N_1 being the smaller, the first term in ν_1 is negligible—in other words, the true secular acceleration of the sun is very small. So long as intervals of only some thousands of years are considered, again, ξ and ξ_1 can be put equal to 1. Put also

$$\frac{Mm}{M+m}\frac{{c_0}^2 n_0}{C\Omega} = \kappa \quad . \quad . \quad . \quad . \quad (10)$$

Then

$$\nu = (\kappa - 3)\frac{M+m}{Mm}\frac{N}{{c_0}^2} + \frac{n_0}{C\Omega}N_1 \quad . \quad . \quad . \quad (11)$$

$$\nu_1 = \kappa\frac{M+m}{Mm}\frac{n_{10}}{n_0}\frac{N+N_1}{{c_0}^2} \quad . \quad . \quad . \quad . \quad (12)$$

The rate of dissipation of energy is

$$-\frac{dE}{dt} = (N+N_1)\Omega - Nn - N_1 n_1 \quad . \quad . \quad . \quad (13)$$

Observation indicates that $\nu = 9''{\cdot}0 \pm 1''{\cdot}40$ per century per century,*

$$\nu_1 = 3''{\cdot}86 \pm 0''{\cdot}54 \text{ per century per century.}\dagger$$

If we could be certain that the secular accelerations contain no part due to other causes than tidal friction, equations (11) and (12) might be solved for N and N_1; dE/dt could then be determined

* J. K. Fotheringham and Miss G. Longbottom, *Monthly Notices of R.A.S.*, 75, 1915, p. 393.

† J. K. Fotheringham, *Monthly Notices of R.A.S.*, 78, 1918, p. 423.

directly. In view, however, of the possibility of error in the determination of the solar secular acceleration, it is probably better to calculate the ratio of N_1 to N from theoretical considerations, and then to find both from the equation giving the secular acceleration of the moon; the equation for the sun can then be used as a check.

First, suppose tidal friction to be almost entirely bodily; then most of it would arise from the lunar and solar semidiurnal tides, which are of roughly equal periods and would lag by almost equal amounts. Thus the couples introduced would be proportional to the products of the heights of the tides into the disturbing potentials; or practically to the squares of the disturbing potentials, so long as the free periods are such that approximate resonance does not occur, which is justified. The ratio of the potentials is 2·3, in accordance with the usual theory, and $N/N_1 = 5{\cdot}1$. Further, $\kappa = 4{\cdot}78$, within probably 2% of its amount. Substituting in (11), (12), and (13) we find

$$\frac{N}{Ma^2} = 7{\cdot}1 \times 10^{-23} \div (1 \text{ sec.})^2 \quad . \quad . \quad . \quad . \quad (14)$$

$$\nu_1 = 1''{\cdot}44 \pm 0''{\cdot}22 \text{ per century per century} \quad . \quad . \quad (15)$$

$$-\frac{dE}{dt} = 1{\cdot}41 \times 10^{19} \text{ ergs per second} \quad . \quad . \quad . \quad (16)$$

Secondly, suppose that all the dissipation is in regions of strong tidal currents, as G. I. Taylor suggests. On account of the closeness of the periods and the narrowness of the channel the lunar and solar tidal currents at any point are in nearly the same direction. The frictional force being proportional to the square of the resultant velocity, this gives a means of determining the ratio of the frictional couples.

At any point let U be the velocity of the lunar current, and V that of the solar current. Suppose the origin of time so chosen that we can write

$$U = A \sin pt\,; \; V = B \sin qt \quad . \quad . \quad . \quad (17)$$

Then B is considerably less than A, and their ratio is approximately the same all over the earth. Terms of other periods are also present; their neglect is equivalent to ignoring B^2. The frictional force F is $k(U+V)^2$, directed against the resultant velocity; or writing it in a form whose direction will change sign automatically when U + V changes sign,

$$F = k|U+V|(U+V) \quad . \quad . \quad . \quad (18)$$

where $|U+V|$ denotes the absolute value of U + V.

The rate of dissipation of energy is F(U + V), or $k|U+V|^3$. The secular frictional couple arising from the lunar tides is given by the part of F with a period equal to half a lunar day; and

similarly for the solar tides. Let these two parts of F be P sin pt and Q sin qt. Then

$$\pi p\mathrm{P} = \operatorname{Lim}_{n\to\infty} \frac{1}{n}\int_0^{2n\pi} \mathrm{F}\sin pt\, dpt \quad . \quad . \quad . \quad (19)$$

$$\pi p\mathrm{Q} = \operatorname{Lim}_{n\to\infty} \frac{1}{n}\int_0^{2n\pi} \mathrm{F}\sin qt\, dpt \quad . \quad . \quad . \quad (20)$$

These periodic frictional forces of the sea on the earth correspond to reactions whose periods agree with those of the two semidiurnal tides and which therefore alter by fixed amounts the phases of these tides. The frictional couples are proportional to the products of the squares of the tide-generating potentials and these phase changes; thus

$$\frac{\mathrm{N}}{\mathrm{N}_1} = \frac{m}{\mathrm{M}+m}\,\frac{n^2}{n_1^2}\cdot\frac{\mathrm{P}}{\mathrm{Q}} = 2{\cdot}3\frac{\mathrm{P}}{\mathrm{Q}} \quad . \quad . \quad . \quad (21)$$

Put

$$pt = \theta,\ q/p = r \quad . \quad . \quad . \quad . \quad (22)$$

then

$$\pi p\mathrm{P} = \operatorname{Lim}\frac{1}{n}\int_0^{2n\pi} k\,|\,\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta\,|\,(\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta)\sin\theta d\theta \quad . \quad (23)$$

$$\pi p\mathrm{Q} = \operatorname{Lim}\frac{1}{n}\int_0^{2n\pi} k\,|\,\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta\,|\,(\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta)\sin r\theta d\theta \quad (24)$$

As B is less than A, the $(m+1)$ zero of $\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta$ will always lie between $(m-\frac{1}{2})\pi$ and $(m+\frac{1}{2})\pi$. Let this be θ_m. Then

$$\pi p\mathrm{P} = \operatorname{Lim}\frac{k}{n}\sum_{m=0}^{2n}(-1)^m\int_{\theta_m}^{\theta_{m+1}} (\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta)^2\sin\theta d\theta = \frac{8}{3}k\mathrm{A}^2 \quad . \quad (25)$$

$$\pi p\mathrm{Q} = \operatorname{Lim}\frac{k}{n}\sum_{m=0}^{2n}(-1)^m\int_{\theta_m}^{\theta_{m+1}} (\mathrm{A}\sin\theta + \mathrm{B}\sin r\theta)^2\sin r\theta d\theta$$

$$\theta_m = m\pi + \frac{\mathrm{B}}{\mathrm{A}}(-1)^m\sin rm\pi,$$

where B^2 has been neglected. Integrating we find that the general term in the sum is

$$\tfrac{1}{2}(-1)^{m-1}\Bigg[\mathrm{A}^2\left\{\frac{1}{r}\cos r\theta - \frac{1}{2(2+r)}\cos(2+r)\theta + \frac{1}{2(2-r)}\cos(2-r)\theta\right\} + 2\mathrm{AB}\left\{\cos\theta - \frac{1}{2(2r+1)}\cos(2r+1)\theta + \frac{1}{2(2r-1)}\cos(2r-1)\theta\right\} + \text{term in } \mathrm{B}^2\Bigg]_{\theta_m}^{\theta_{m+1}}.$$

When we substitute in the trigonometrical terms the values of θ at the limits, we have to the first order in B

$$\tfrac{1}{2}(-1)^{m-1}\Big[\frac{A^2}{r}\{\cos r(m+1)\pi - \cos rm\pi\}$$
$$-\frac{A^2}{2(2+r)}\{\cos(2+r)(m+1)\pi - \cos(2+r)m\pi\}$$
$$+\frac{A^2}{2(2-r)}\{\cos(2-r)(m+1)\pi - \cos(2-r)m\pi\}\Big],$$

$$+\tfrac{1}{2}AB[\{1-\tfrac{1}{2}\cos 2r(m+1)\pi + \tfrac{1}{2}\cos 2rm\pi\}$$
$$-\tfrac{1}{2}\{1-\tfrac{1}{2}\cos 2(1+r)(m+1)\pi + \tfrac{1}{2}\cos 2(1+r)m\pi\}$$
$$-\tfrac{1}{2}\{1-\tfrac{1}{2}\cos 2(1-r)(m+1)\pi + \cos 2(1-r)m\pi\}]$$

$$+2AB\Big[1-\frac{(-1)^{m-1}}{4(2r+1)}\{\cos(2r+1)(m+1)\pi - \cos(2r+1)m\pi\}$$
$$+\frac{(-1)^{m-1}}{4(2r-1)}\{\cos(2r-1)(m+1)\pi - \cos(2r-1)m\pi\}\Big].$$

When the terms of this character are summed from $m=0$ to $m=2n-1$, where n is large, this will give the approximate value of the integral from $\theta=0$ to $2n\pi$. The terms with r in the argument form several series of sines and cosines of angles in arithmetic progression, and their sum is, therefore, never greater than a definite amount. Hence the integral is approximately $2AB(2n+\lambda)$ where λ does not tend to infinity with n, and $\pi pQ = 4kAB$. (26)

The effective part of the frictional force will be of the form

$$\frac{8}{3}\frac{kA}{\pi p}(A\sin pt + \frac{3}{2}B\sin qt) \quad . \quad . \quad . \quad (27)$$

Hence
$$N/N_1 = A^2/\tfrac{3}{2}B^2$$
$$= 3{\cdot}4 \quad . \quad . \quad . \quad . \quad . \quad (28)$$

if A/B is not much altered by local resonance effects, which is probably true on the whole. Thus the solar tidal friction is a larger fraction of the lunar than on the theory of bodily friction. We find now

$$\frac{N}{Ma^2} = 6{\cdot}1\times 10^{-23} \div (1\text{ sec.})^2 \quad . \quad . \quad . \quad . \quad (29)$$

$$\nu_1 = 1''{\cdot}56 \pm 0''{\cdot}24 \text{ per century per century} \quad . \quad (30)$$

$$-\frac{dE}{dt} = 1{\cdot}38\times 10^{19} \text{ ergs per second} \quad . \quad . \quad . \quad (31)$$

The water surface of the earth being $3{\cdot}67\times 10^{18}$ sq. cm., the average dissipation of energy over the whole of it is, therefore, 3·7 ergs per square centimetre per second. The calculated and observed solar secular accelerations differ by more than three times

the sum of their probable errors. This suggests that either an unknown cause is producing an acceleration of the sun or that about half of the observed ν_1 is error.

At the same time the effect of friction on the 14-monthly variation of latitude can be estimated, for the above treatment of the frictional terms is independent of the ratio of the periods of the motions concerned. The coefficients in the disturbing potentials being, say, X and Y, the rates of dissipation of energy are in the ratio $A^2/\frac{2}{3}B^2$, B now referring to the 14-monthly tide and not to the solar tide. As the periods are now very different, we can no longer assume $A/B = X/Y$; but the heights of the respective equilibrium tides are in the ratio of X/Y. The configuration of the ocean bed causes A to be much greater than its value on the equilibrium theory; at the same time B is probably little different from its equilibrium value as the movement is so slow. Hence A/B is greater than pX/qY, and λ, the ratio of the rates of dissipation of energy, is greater than $\frac{2p^2X^2}{3q^2Y^2}$.

Let now the earth's axis of rotation have direction cosines $(l, m, 1)$ relative to its mean position in space. The disturbing potential due to the change of position of the axis of rotation is $-\Omega^2 z(lx+my)$, and the height of the equilibrium tide is of order $\Omega^2 a^2 l/g$, or $2\times10^6 l$ cm. That of the lunar equilibrium tide is of order 30 cm., so that the ratio X/Y is of order $1/10^5 l$. Hence λ is greater than $1/10^4 l^2$, making the rate of dissipation of energy in the variation of latitude less than $5\times10^{23} l^2$ ergs per second.

The energy actually present in the variation of latitude is of order $\frac{C^2}{(C-A)}(l^2+m^2)\Omega^2$, or $6\times10^{38} l^2$ ergs. It would, therefore, be dissipated in a time greater than 10^{14} seconds, or 10^7 years, if tidal friction in partly enclosed seas were the only agency tending to reduce it. The existence of a 14-monthly variation of latitude therefore offers no obstacle to the theory. At the same time, if the whole of the secular acceleration can be attributed to this type of friction, it will be allowable to treat the whole earth as perfectly elastic for disturbances of periods of the order of a day.

The calculation of the dissipation must be regarded as rough. The theoretical ratio of the spring and neap tides would be 3·3 : 1·3, or 2·5 nearly. Actually the ratio is rather less, about 2, and this reduction seems universal. It is to be attributed to the greater influence of friction in reducing the tide when the velocity is greatest. This must at the same time introduce into the velocities many considerable terms of different periods from those yet considered, and equations (23) and (24) should be modified so as to include these. The error is probably of the same order as that caused by neglecting B^2. It therefore hardly seems worth while to attempt to correct one of these without the other; the numbers found may as well be at present accepted as they stand, though each is probably in error by about a third of its amount.

An Equatorial Mounting for Eclipse Observations. By C. Davidson and W. V. Woodman.

(*Communicated by the Astronomer Royal.*)

The result obtained at the recent eclipse is of such a remarkable character that its verification at the earliest opportunity is imperative. The figures $1''{\cdot}98 \pm ''{\cdot}12$ are sufficiently near the $1''{\cdot}75$ predicted by Einstein to be considered confirmation of the relativity theory, providing there are no rival explanations. Alternatives have, however, been put forward and must be considered, as it is not impossible that some part of the observed displacement may be due to other causes, such as refraction, either by the sun's or by the earth's atmosphere, or by both. The observation is not sufficiently exact to enable this to be investigated. It will be necessary to obtain fresh observations made with the greatest possible refinement, so that the probable error may be reduced to the smallest limits.

Both at Sobral and Principe cœlostats were used in making the observations. These were originally designed to permit of the use of long-focus telescopes without the equatorial mounting otherwise necessary. They possess undeniable advantages for such eclipse work as photography of the corona, and for use with spectrographs, but for this problem, which requires the positions of stars to be determined with the highest order of accuracy attainable, they are not so suitable. The use of the mirror is undesirable, as an additional and somewhat unstable element is introduced into the optical train.

At the next opportunity when the Einstein effect may be observed, it is suggested that the observations should be made with a telescope equatorially mounted, and to this end a mounting has been designed on the plan of the English equatorial suitable for carrying the 13-inch astrographic telescope, with which it is believed photographs of the necessary quality could be obtained at the eclipses of 1922 and 1923, although in neither case is the field of stars near the sun particularly favourable for the purpose.

There is no particular originality in the design which is here put forward for constructive criticism. The objective aimed at is simply the possibility of mounting a telescope which, whilst rigid and capable of refined work, should also be so constructed that it would be possible to erect it at a station where skilled labour was not obtainable. Where vital parts have to be taken to pieces, provision is made so that when reassembled the instrument will be in as perfect alignment as when first constructed. This relates especially to the polar axis, which consists of four pieces bolted together and machined as one piece.

The tube of the telescope is in three sections, the central being a casting carrying trunnions which form the declination pivots; the upper and lower sections are light steel tubes bolted to it. This is mounted in an open rectangular frame 4 feet in length,

which forms the polar axis. The polar axis is constructed of four pieces—two side cheeks in which the declination axes are pivoted, and two ends, bolted to the cheeks, which carry the polar pivots. These pivots rest in self-aligning bearings carried on two piers, the telescope swinging between them. The bearings are relieved of the greater part of the weight by the use of ball races with suitable adjustments, and the down thrust is taken off the lower bearing by a ball thrust, also with suitable adjustment. A driving arc is attached to the upper axis outside the pier, and the clock of the astrographic equatorial can be adapted to drive it. The change of direction of motion for north and south latitude is made by means of contrate wheels on the adjustable shaft connecting the clock to the driving screw. It is proposed that the piers be constructed of angle iron, which, when erected and adjusted, could be made solid by filling in with earth or concrete. The piers of the model are designed for Christmas Island, lat. 11° south. It may be adapted to other latitudes by introducing iron castings of the proper angle.

The dimensions admit of practically every part being within easy reach.

Height of angle-iron piers	4 feet
„ „ upper polar axis	7 „
„ „ lower polar axis	6 „
„ „ declination axis	6½ „
Space between piers	4 „
Least distance plate-holder to ground . .	1½ „

In erecting the instruments the following procedure would be observed:—

Having chosen a suitable site and levelled it, determine and set out the meridian carefully. The angle-iron piers are erected in position, levelled, and the meridian verified. The latitude castings may now be bolted on, then the casting carrying the driving arc and the bearings of the polar axis. A wooden template, representing the polar axis, carrying a small telescope or theodolite, is now put in place, the bearings carefully adjusted, and azimuth and elevation determined and adjusted. The polar axis with the central section of the telescope is now put together and the whole lifted into position by tackle, or a platform might be built between the piers, of a suitable height, and the polar axis put together in position in its bearings. The telescope tube is now completed by bolting on the other sections. When the arc has been placed on the upper polar pivot, the clock is mounted on the platform provided for it. The adjustment of the polar axis having been brought within the range of the screws provided, the skeleton piers may be filled in with sand, earth, or concrete, as may be convenient.

Optical Systems with Non-spherical Surfaces.

By A. E. Conrady.

Non-spherical surfaces have been discussed and used in connection with optical instruments ever since the early days of the telescope. The parabolic mirror of reflecting telescopes was probably the first example, followed by the elliptical small concave mirror of the Gregorian and the hyperbolical small convex mirror of the Cassegrainian reflector. Removal of the spherical aberration for the axial image-point was in each case the object aimed at and attained. Similar investigations concerning refraction at curved surfaces soon followed and led to the Cartesian ovals as the generating line for the surfaces of lenses free from axial spherical aberration at every surface.

For the simple reason that the natural tendency of the abrading processes used in the finishing of optical elements is to produce true spherical surfaces and that deliberate departures from this form greatly increase the difficulty and cost of production, the use of "figured" surfaces has been avoided as far as possible in the wholesale manufacture of optical instruments, the perfection of all of which depends largely, or more often absolutely, upon the skilful compensation of antagonistic spherical aberrations at successive strictly spherical surfaces. In recent years practical opticians have, however, again become interested in this question of departing from strictly spherical surfaces, and a few successful attempts in special forms of spectacle lenses and microscope condensers have stimulated this newly awakened interest and have led in certain quarters to extremely sanguine hopes of attaining high perfection of optical instruments with far simpler means than those which were found indispensable under the restriction to exactly spherical surfaces.

I do not think that these hopes will be realised to anything like the anticipated extent. In refracting instruments there are the two forms of chromatic aberration (of focal point and of focal length) which necessitate the combination of two or three lenses for their removal, and which are not affected to any sensible extent by any practically conceivable amount of figuring. Secondly, there is the sine-condition which must be fulfilled in order to secure reasonably sharp definition in the central part of the field of view, and here again it was pointed out in my proof of the condition (*Monthly Notices*, 65, 501 (1905)) that it holds equally for spherical and for figured surfaces and that it prescribes definite forms of a lens system. For the ordinary telescope objectives there is thus no hope of any startling improvement by the deliberate employment of figured surfaces.

In the more deeply curved lens-systems with a field of small angular extent, of which the more powerful microscope objectives are the most important representatives, the components have already been carried—with spherical surfaces—to such a depth of

curvature that any great increase is out of the question, and a diminution of their number is, therefore, hardly possible. As it has also been found possible to reduce the residual aberration to the Rayleigh limit, we again see little prospect of substantial simplifications.

This leaves only systems with a field of large angular extent—photographic objectives and eyepieces—to be discussed. In their case the aberrations of oblique pencils, primarily the five Seidel aberrations, play the most important part, and great expectations are entertained as to the effect of non-spherical surfaces in removing these aberrations more completely and with simpler means.

I now propose to demonstrate that whilst there are here some possibilities, they do not go nearly so far as seems to have been anticipated, and that all the advantage to be gained arises from the fact that by the use of "figured" surfaces we can give to a lens or combination of lenses any prescribed amount of spherical aberration *for the axial image-point* with perfect freedom as to its gross external form. I shall show that the three fundamental equations of oblique pencils given in my last paper (1919 November) remain in full force regardless of whether the lens-surfaces are spherical or "figured."

With regard to the Seidel-aberrations, the proof that they apply to figured surfaces is really contained in Prof. R. A. Sampson's "New Treatment of Optical Aberrations" (*Phil. Trans.*, Ser. A, 212, pp. 149-185), and the inclusion of this valuable generalisation appears to me to be the most remarkable feature of that highly interesting paper.

The following modification of my proof of the Seidel-aberrations (*Monthly Notices*, 79, 60 (1918)), which simplifies the original one and generalises it at the same time, appears, however, worthy of being recorded, as it clearly establishes the separate effects of the spherical surfaces and of any superposed figuring and the relations of the aberrations to the Petzval-theorem, which latter is proved to remain in full force for figured surfaces.

The modification of the proof consists in substituting the differences of optical paths arising at the different surfaces for the lateral or transverse spherical aberration previously employed. In my paper on the spherical correction of object-glasses (*Monthly Notices*, 65, 594 (1905)) it was shown that the aberration at a spherical surface causes an acceleration (in the case of under-correction) of the marginal parts of a wave which grows in first approximation with the fourth power of the aperture; the lead with which the light from a zone of radius y arrives at the paraxial focus is, therefore, given by

$$(1) \quad . \quad . \quad . \quad . \quad W_s = k \cdot y^4,$$

in which k is the constant of the wave-distortion for the particular surface and conjugate distances; the explicit value of this constant was given in the 1905 paper. At any one spherical surface this constant applies equally for points on the principal optical axis

and for those on any auxiliary optical axis passing through the centre of curvature.

In any centred optical system only surfaces of rotation with axis coinciding with the principal optical axis can come into question. Such a surface can depart from a spherical surface of the same axial radius of curvature only by amounts proportional to the fourth and higher *even* powers of the distance from the optical axis. The resulting change in the level of the surface will cause an algebraical shortening of the optical path equal to $(N' - N)$ times the depression of the figured surface below the spherical one, N' and N being the refractive indices of the media beyond and before the surface.

The effect of figuring will, therefore, be represented to the order of the primary aberrations by the equation

$$(2) \qquad . \quad . \quad . \quad . \quad Wf = h \,.\, y^4,$$

in which h is an absolute constant not depending on the radius of

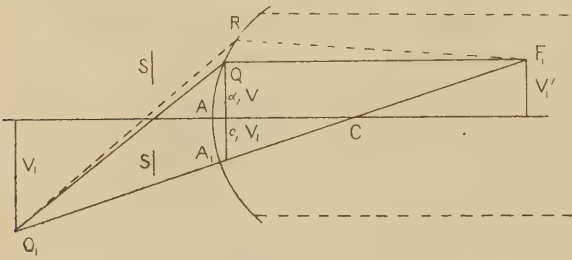


FIG. 1.

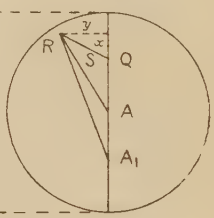


FIG. 2.

curvature or on the conjugate distances, but the y must be measured from the *principal* optical axis.

If we now refer to the diagrams, figs. 1 and 2, closely corresponding to those in the paper of 1918 November, and consider an extra-axial object-point O_1 at distance V_1 from the principal optical axis, we can still draw the auxiliary axis O_1C through the centre of axial curvature of our figured surface and can still refer the phenomena to the point F_1 at distance $V_1{}^1$ from the principal axis at which a thin radial pencil from O_1 through a strictly spherical surface of radius AC would come to a focus. Any number of following surfaces can be treated in the same way, and the final image found from this succession of thin radial pencils will conform to the Petzval-curve calculated with the axial curvatures of the figured surfaces. We can then apply equations (1) and (2) to any ray from O_1 and determine the difference of optical path with which it will arrive at F_1.

Let there be a diaphragm, or in the general case let an image of the actual diaphragm, or "entrance-pupil," be located at SS, marking the point where all the oblique cones of rays have a common base. The ray from O_1 through the centre of this

aperture will reach the refracting surface at Q. In accordance with (1) this ray would, at a spherical surface, suffer an acceleration relative to O_1F_1 of k_1 times the fourth power of the perpendicular distance from Q to O_1F_1, but as this distance as well as the various angles of obliquity must be looked upon as a small quantity of the first order, it differs only by a small quantity of the third order from the corresponding distance measured along a perpendicular to the principal optical axis, and the fourth power of the latter perpendicular would differ from that of the correct perpendicular only by a sixth order term, which, therefore, is of the order of the *higher* aberration neglected in this investigation. This simplification and others of the same type apply throughout, and should be borne in mind.

It is at once obvious from fig. 1 that the part of the adopted distance which lies above the optical axis is proportional to V_1, and may be put $= d_1 . V_1$, and the part below the optical axis is also proportional to V_1, but in a ratio which may be very different in magnitude and may (as in our case) be of opposite sign, we will put this $= c_1 V_1$. We can now also apply (2) and state that the figuring of the surface will accelerate the ray at Q by $h_1 d_1^4 V_1^4$, and the radial ray at A_1 by $h_1 c_1^4 V_1^4$.

We now take the end view of our surface in fig. 2, in which A_1Q marks the plane containing the optical axis and the auxiliary axis, and consider any ray penetrating the surface at R. We will define R by rectangular co-ordinates x_1 and y_1 referred to Q. Joining R with A and with A_1 we shall have

$$Ws_1 = k_1 . A_1R^4$$
$$Wf_1 = h_1 . AR^4.$$

Fig. 2 gives at sight

$$(A_1R)^2 = ((c_1 + d_1)V_1 + x_1)^2 + y_1^2$$
$$(AR)^2 = (d_1V_1 + x_1)^2 + y_1^2,$$

and by squaring these again and introducing them into the equations for Ws_1 and Wf_1 we obtain

$$(3)\quad \begin{cases} Ws_1 = k_1(x_1^2 + y_1^2)^2 + 4k_1(c_1 + d_1)x_1(x_1^2 + y_1^2)V_1 \\ \qquad\qquad + 2k_1(c_1 + d_1)^2(3x_1^2 + y_1^2)V_1^2 \\ \qquad\qquad + 4k_1(c_1 + d_1)^3x_1V_1^3 + k_1(c_1 + d_1)^4V_1^4 \\ Wf_1 = h_1(x_1^2 + y_1^2)^2 + 4h_1d_1x_1(x_1^2 + y_1^2)V_1 \\ \qquad\qquad + 2h_1d_1^2(3x_1^2 + y_1^2)V_1^2 \\ \qquad\qquad + 4h_1d_1^3x_1V_1^3 + h_1d_1^4V_1^4. \end{cases}$$

The addition of these two gives the total acceleration of the light passing along the ray through R with reference to the light passing radially from O_1 to F_1, but as yet on the supposition that the latter light passes through the strictly spherical surface. The figuring of the latter will accelerate the radial light by the amount $h_1c_1^4V_1^4$ already determined, and the accurate net acceleration with

which the marginal light meets the radial light at F_1 will therefore be $= Ws_1 + Wf_1 - h_1c_1^4V_1^4$.

Every surface will produce an acceleration of this type, with its own values of h, k, c, and d, all of which may vary greatly in value and even in sign for different surfaces, but for objects at a given distance are constants for any one surface. The value of V at the several surfaces varies according to the position of the original object-point and according to the magnification of the intermediate image which is presented to each surface as the object to be depicted by it. All the V will be proportional to that of the original object-point and may be put $= q_r V$, where q_r will be a constant for the rth surface, and V is taken as the symbol for the distance of either the original object-point or, at pleasure, for that of any selected image-point, preferably the final one, from the optical axis. The x and y will also vary—usually to a modest extent, according to the convergence in the space between successive surfaces of the selected ray with reference to the principal ray passing through the centre of the diaphragm. But under the necessary restriction to small aperture and field and consequently to small aberrations x and y will at all the surfaces bear the same ratio to each other and each one will be proportional to its value in a selected position, either in the diaphragm or preferably, for practical reasons, in the final image of the diaphragm, the "exit-pupil." Using plain x and y as the symbols for the co-ordinates in the selected position, say, in the exit-pupil, we may therefore put $x_r = p_r x$ and $y_r = p_r y$, where p_r is against a constant for the rth surface.

The result of substituting these values will be that we obtain the total acceleration at any surface in the form of a sum of six terms, each of which consists of a factor, constant for that surface, multiplied into a product of powers of V, x, and y, which is the same for all surfaces. As the accelerations combine by simple algebraical addition, the total relative acceleration with which any ray arrives at the final focus of the succession of thin radial pencils will therefore be of the form

$$\text{I.}\quad W = a_1(x^2+y^2)^2 + a_2x(x^2+y^2)V + a_3(3x^2+y^2)V^2 + a_5xV^3 + a_6V^4.$$

The constant coefficients a have been numbered to correspond to Seidel's sequence. The supernumerary last term in y^4 is quite devoid of significance. Being entirely independent of x and y, it has the same value for the whole clear aperture and therefore does not imply any distortion of the issuing waves from the ideal spherical form. It simply represents the aggregate of the accelerations with which all the light really passing through the lenses meets the successive focal points F of the corresponding radial pencils. As these successive radial pencils are really discontinuous and merely form a very convenient *pons asinorum*, no physical interest of any kind is attached to the term a_6V^4, and we shall disregard it entirely.

The combination of the accelerations arising at the several

surfaces by simple addition really implies the assumption that the light travels from surface to surface in the direction towards the focal point F of the corresponding radial pencil. This is not rigorously correct, for the light is propagated in the direction of the geometrical rays as determined by the law of refraction, and this direction differs from the assumed direction by the amount of the angular aberration. The latter, however, is a small quantity of the third order, and as the geometrically determined path has, by Fermat's theorem, a stationary value, its difference from neighbouring paths forming third-order angles with it is a small quantity of the sixth or higher order. Therefore the correction neglected in the simple addition of the accelerations is of the order of the higher aberrations, and its omission is justified.

We will now show that the W defined by equation I., with omission of the meaningless last term, agrees with the transverse

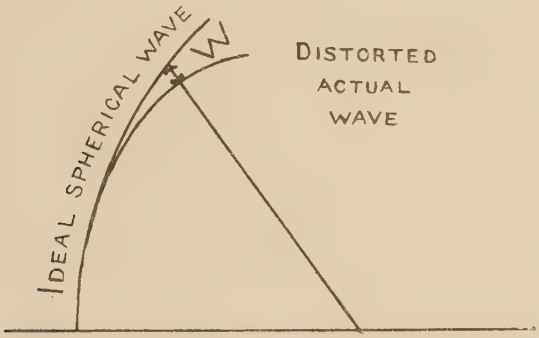


FIG. 3.

displacements of the issuing rays used in my previous papers, and we will establish the connection between the coefficients a and the m-coefficients of the transverse displacements. The W of equation I. measures the gap, at the distance from the principal ray defined by x and y, between the ideal spherical wave converging towards the focus of the last radial pencil in the Petzval-surface and the actual wave distorted by the aberrations (see fig. 3), the gap being measured as an optical path: should the final image be formed in a medium of refractive index N' (as was the case in Airy's telescope filled with water for his famous experiment concerning stellar aberration), W must therefore be divided by N' to convert it into a geometrical length, and we will include this divisor in order to preserve perfect generality. Then $\partial W/N'\partial x$ will represent the angle between a section parallel to the plane of the optical axis of the ideal and the actual wave and $\partial W/N'\partial y$ will measure the corresponding angle in a section at right angles to the plane of the optical axis. As the ideal and actual rays are normals of the respective waves, these two differential coefficients also express the components of the angular aberration in the x and y

directions. If the distance from the pupil—the exit-pupil having been recommended for the purpose—in which the x and y are measured to the final focus is P, then these components will produce transverse displacements of P times the respective differential coefficients, and these evidently represent the horizontal displacement (measured from the focus of the final radial pencil radially towards the centre of the field) and vertical displacement (measured tangentially) of the actual ray in the Petzval-surface and are identical with the displacements dealt with in my previous papers. They are thus found as

$$\text{Horiz. Displ.} = \frac{P}{N'}[4a_1x(x^2+y^2) + a_2V(3x^2+y^2) + 6a_3V^2x + a_5V^3]$$

$$\text{Vert. Displ.} = \frac{P}{N'}[4a_1y(x^2+y^2) + 2a_2Vxy + 2a_3V^2y].$$

We can now change from rectangular co-ordinates to the polar ones previously used by putting

$$x = S\cos E \qquad y = S\sin E \qquad x^2+y^2 = S^2$$

and obtain

$$\text{Horiz. Displ.} = \frac{P}{N'}[4a_1S^3\cos E + a_2S^2V(2+\cos 2E) + 6a_3SV^2\cos E + a_5V^3]$$

$$\text{Vert. Displ.} = \frac{P}{N'}[4a_1S^3\sin E + a_2S^2V\sin 2E + 2a_3SV^2\cos E].$$

Comparing these with the equations (d) of the paper of 1919 November:

$$\text{Horiz. Displ.} = m_1S^3\cos E + m_2S^2V(2+\cos 2E) + 3m_3SV^2\cos E + m_5V^3$$

$$\text{Vert. Displ.} = m_1S^3\sin E + m_2S^2V\sin 2E + m_3SV^2\sin E,$$

and bearing in mind that the m-values are most easily determined numerically by the trigonometrical calculations indicated in the quoted paper, we deduce the corresponding a-values for the evaluation of the distortion of the final emerging waves of lens-systems:

$$\text{II.} \qquad a_1 = m_1\frac{N'}{4P} \qquad a_2 = m_2\frac{N'}{P} \qquad a_3 = m_3\frac{N'}{2P} \qquad a_5 = m_5\frac{N'}{P}.$$

These simple relations will prove extremely valuable to the practical designer, as they supply the means of easily estimating the maximum aperture and field for which a given lens-system will give a confusion of phase within any prescribed limit, say, the Rayleigh limit of $\frac{1}{4}$ wave-length, and thus to avoid the highly misleading purely geometrical discussion of the distribution of the "rays" to which I have called attention in previous papers.

For such purposes it will generally be preferable to transform equation I. into the polar co-ordinates S and E: it thus becomes

IA. $$W = a_1S^4 + a_2S^3V\cos E + a_3S^2V^2(2 + \cos 2E) + a_5SV^3\cos E.$$

Either I. or IA. define the wave-form corresponding to the separate Seidel-aberrations and supply the starting-point for integrations to determine the true light-distribution. Those for spherical aberration and coma were thus employed in my paper on "Star-discs" (*Monthly Notices*, **79**, 575).

We must, however, return to the proper subject of the present paper. As we have proved the absolute analytical identity of the aberrations arising at figured surfaces with those previously studied under restriction to spherical surfaces, it is clear that the Petzval-theorem, both in its original and in the extended form of equation I. of the paper of 1919 November, holds absolutely for figured surfaces. Therefore the use of the latter will not enable us to attain anastigmatic flattening of the field unless the radii of curvature and refractive indices of the component lenses lead to a zero-value of the simple Petzval-equation, and the three to one ratio of the distances of tangential and sagittal focal lines from the Petzval-curve also remains undisturbed.

Equation II. of the paper of *Monthly Notices*, **80**, 78, which shows the effect of longitudinal shifts of the diaphragm, was obtained by a simple transformation of co-ordinates and cannot be affected in any way by the *origin* of the final aberrations. Hence this important equation, with all the deductions derived from it, also applies to optical systems containing figured surfaces and is thus universally valid.

In the case of equation III. of the previous paper, which fixes the astigmatism and curvature of field of thin lens-systems passed centrally by the oblique pencils, the proof of the equation depends only on the paraxial curvature of the surfaces and is, therefore, quite independent of any departure from strict spherical form in the outer zones of the surfaces.

We thus conclude that all the deductions of the paper of *Monthly Notices*, **80**, 78, are applicable to systems with non-spherical surfaces. The effect of figuring in the case of thin systems passed centrally by the oblique pencils is most conclusively shown by equations (3) of the present paper. Central passage of the oblique pencils obviously means $\delta l = 0$, and if this value is put into the equation for Wf_1, which displays the effect of the figuring, all terms with the exception of the first vanish. The latter term manifestly represents pure spherical aberration, and a change in the amount of this aberration is, therefore, the only direct effect of the departure from strictly spherical surfaces.

We may, in fact, say that the sole advantage of figured surfaces lies in the direct control which it gives us over the ordinary spherical aberration, which may be brought to any desired positive or negative value by removing a layer of glass only a moderate number of wave-lengths in thickness in suitable zones of one or

more surfaces without any visible change in the gross form of the lens or system. Occasionally this may enable us to save one component in a compound-lens, but the chief advantage will be that the independent control over spherical aberration will render it possible to satisfy other conditions in a more desirable manner —for instance, with smaller separations between separated components than would be possible if spherical surfaces were alone admitted. In the case, for example, of the Petzval portrait-lens used as an illustration in the latter part of the 1919 November paper, the requirement that the front component must supply eccentric pencils with a definite amount of overcorrected astigmatism and with next to no distortion to the central part of the back-lens will, by equation II. of that paper, lead to a demand for a perfectly definite amount of spherical aberration and of coma in the pencils passing centrally through the front component. Under restriction to spherical surfaces the required amounts of the two aberrations will only be obtainable with a very large separation between front- and back-components if the front-lens is to be a cemented one of desirable kinds of glass. If a figured surface is admitted we can obtain the desired amount of coma unconditionally by "bending" the cemented front-component, and we can then figure the latter to secure the requisite spherical aberration. By the use of another figured surface in the back-component it would also be possible to cement the latter instead of retaining Petzval's airgap and thus to secure greater light-transmitting power and a great diminution of doubly reflected false light.

Technical difficulties will, however, impose rather severe restrictions upon the extent to which figuring may be carried, for if the amounts are large it will not only be difficult to vary the thickness of the removed layer of glass sufficiently accurately in accordance with the theory, but there will also be the additional difficulty of centring the figuring with reference to the optical axis of the finished instrument.

For these reasons it does not appear very probable that *heavily* figured lenses will ever be used in instruments of precision, in spite of the fact that considerable improvements in compactness are undeniably within reach by the employment of such lenses, and that they may occasionally enable the designer to save one or two component lenses.

Imperial College of Science and Technology:
1920 *January* 15.

Micrometrical Measures of Double Stars (*14th Series*) *and New Double Stars.* By the Rev. T. E. Espin.

In the following list the measures have been made with the 24-in. Calver reflector. Stars measured by Mr. Milburn are followed by the letter M:—

Micrometrical Measures.

Name.	R.A. 1900.	Decl.	P.A.	D.	Mags.	Nts.	Date.
	h m	° ′	°	″			1919.
η Cassiop.	0 43·0	+57 17	255·1	7·11	3·0 9·0	3	·004
Σ 192	1 53·8	58 9	180·0	4·74	8·3 9·0	3	·067
Σ 272	2 25·8	58 1	39·7	1·74	8·5 8·6	3	·067
Σ 384	3 20·4	59 33	266·0	1·74	8·3 8·5	3	·122
Holmes	3 21·6	59 34	51·1	5·21	9·0 11·1	3	·122
Espin 55	3 57·8	59 1	260·2	9·66	8·3 13·0	2	·163
Σ 526	4 17·2	60 2	55·7	5·70	7·2 7·6	2	·969
β Cameli Espin 58	4 54·5	60 18	167·7	14·68	7·2 12·2	2	·118 BC
A. 1307	5 28·9	58 29	130·8	4·50	9·0 10·8	2	·118
Σ 881	6 13·1	59 25	111·5	0·79	8·0 9·0	2	·057 AB
			95·9	26·34	C=12·7	2	·691 AC (new)
Σ 946	6 36·0	59 36	127·5	3·86	8·3 9·2	3	·087
Σ 948	6 37·4	59 32	110·5	1·63	6·2 6·4	4	·103 AB
			307·0	8·57	C=7·0	2	·075 AC
OΣ 159	6 48·6	58 33	19·2	0·67	...	3	·140
h 2377	7 19·9	59 2	260·9	18·65	8·6 9·5	2	·235
Σ 2060	16 26·6	56 57	246·5	3·29	8·8 8·8	3	·602
Kr. 46	17 18·2	58 38	67·3	1·81	8·5 8·6	6	·625
Σ 2199	17 36·8	55 48	77·4	1·77	...	2	·608
Ho. 71	17 49·2	55 24	229·6	3·43	10·2 10·6	2	·747
Σ 2278	18 1·1	56 26	148·8	6·20	7·7 8·0	2	·613 BC
			25·2	37·07	A=7·3	2	·613 AB
Espin 188	18 31·9	58 38	219·1	10·54	8·5 13·3	3	·640
Σ 2353 rej.	18 31·9	58 42	257·2	13·84	9·3 11·1	2	·634
Σ 2398	18 41·7	59 28	152·6	16·93	8·6 8·9	2	·624
β 465	18 42·0	56 46	291·7	2·81	8·4 9·5	3	·732
Espin 189	18 42·6	60 33	108·1	3·57	9·4 12·2	3	·648
Σ 2410	18 45·5	59 13	86·3	1·56	8·4 8·5	2	·598
Σ 2420	18 49·7	59 17	331·0	32·57	5·5 8·1	2	·598
Σ 2433	18 55·0	56 35	124·0	7·16	7·0 9·8	3	·732 AB
			128·4	36·30	C=12·5	3	·732 AC

Name.	R.A. 1900.		Decl.		P.A.	D.	Mags.		Nts.	Date.
	h	m	°	′	°	″				1919.
Arg. 33	19	1·9	+57	18	56·6	10·57	8·2	8·6	2	·642
Hn. 34	19	10·3	55	8	267·3	2·74	8·3	8·9	2	·642
h 1384	19	14·5	55	58	145·2	5·96	9·6	9·8	2	·613
Σ 2516	19	19·0	55	38	237·6	4·08	8·6	9·0	2	·642
Σ 2526	19	21·0	56	49	87·6	17·03	7·5	10·5	1	·687 M
					85·7	16·70	8·5	10·0	1	·745
h 2874	19	27·3	57	48	167·7	9·83	9·4	10·8	2	·613
Σ 2554	19	32·4	60	3	196·3	18·38	7·9	8·3	2	·689 M
					194·8	18·66	8·0	8·5	1	·682
h 2889	19	34·1	59	34	164·6	12·04	9·3	11·1	2	·656
Espin 130	19	46·1	60	54	59·2	2·62	9·5	9·7	2	·624
h 2905	19	46·4	60	56	196·3	8·77	8·8	10·6	3	·634
Espin 201	19	57·7	59	28	144·0	4·28	8·5	11·5	3	·611 AB
					98·0	30·57	C=10·5		2	·608 AC
Σ 2623	19	57·7	59	10	104·9	1·58	8·4	9·2	3	·634
h 1556?	20	33·9	55	36	160·7	4·39	9·6	10·4	2	·747 AB
					227·8	7·57	C=	10·7	2	·747 AC
					103·1	27·78	D=	9·7	1	745 AD
Kr. 51	20	42·1	58	40	181·0	1·59	9·0	9·5	2	·689 M
					179·8	1·39	8·8	9·4	2	·678
A 760	21	5·5	58	43	330·2	3·31	9·1	9·3	2	·642
h 1709	21	54·8	56	6	314·2	7·85	8·5	12·0	2	·921
h 1751	22	15·1	55	38	93·3	1·68	9·7	10·0	2	·938 AB
					113·7	10·72	C=	9·7	2	·938 AC
Σ 3037	23	41·3	59	55	217·6	3·00	7·5	8·5	2	·919 AB. M
					216·3	2·79	6·5	8·5	1	·955
					186·9	29·19	C=8·9		2	·919 AC. M
					186·4	29·20	C=9·5		1	·955
					228·8	49·33	D=9·5		2	·919 AD. M
					229·1	50·18	D=10·2		1	·955

Notes.

Kr. 46, R.A. $17^h\ 18^m\cdot2$.—Angle increasing.

h 1556, R.A. $20^h\ 33^m\cdot9$.—Bies. notes the total dissimilarity with *h*'s description. I suspect that *h* 1556 is really *h* 1580, which follows by 10 m. in R.A., or that by mistake the description of *h* 1580 was entered in this place.

A. 760, R.A. $21^h\ 5^m\cdot5$.—Angle misprinted as 339°·3 in β's G.C.

h 1751.—The distances of the faint *comites* should be reversed in Bies. I found 1919·986 for AD, P. 77°·1, D. 24″·24, and noted E as at P. 24°·8, D. 30″±.

Σ 3037, R.A. $23^h\ 41^m\cdot3$.—The *comes* D was added by Mr. Milburn.

New Doubles.

No.	B.D.	R.A. 1900	Decl.	P.A.	D.	Mags.	Nts.	Date.
	°	h m	° ′	°	″			1919.
1769	+57 99	0 27·7	+58 4	94·0	2·80	9·5 10·5	3	·579
1770	...	0 28·7	57 43	341·3	3·31	9·5 9·6	2	·981 AB
				126·8	13·20	C = 12·0	2	·981 AC
1771	57 169	0 49·6	57 27	356·0	7·94	9·3 12·3	3	·871
1772	55 397	1 38·2	55 37	141·1	2·35	9·6 10·0	2	·956 BC
				106·0	24·01	A = 9·0	2	·956 AB
1773	55 399	1 38·4	55 36	334·2	5·16	10·7 11·5	2	·956 BC
				284·7	53·62	A = 8·5	2	·982 AB
1774	58 516	2 39·1	58 17	72·2	3·87	9·5 10·8	2	·165
1775	57 674	2 52·9	58 5	280·8	6·27	9·5 9·8	2	·981
1776	...	2 56·9	58 54	331·5	3·89	10·1 10·5	2	·921
1777	...	3 20·5	56 7	242·9	2·36	10·2 10·3	3	·074
1778	...	3 50·8	59 56	162·6	4·07	9·4 12·2	3	·152
1779	...	5 5·7	57 16	43·6	3·04	9·5 10·5	3	·166
...	58 884	5 50·7	58 52	46·7	12·24	8·6 9·0	2	·139
1780	58 905	6 5·6	58 34	333·5	3·08	9·4 13·3	4	·161
1781	59 1169	8 20·7	59 4	301·7	4·23	9·4 10·0	2	·163
1782	59 1216	8 56·2	59 9	332·7	3·30	9·1 11·2	3	·214
1783	...	9 29·3	58 21	10·6	2·29	9·8 10·7	3	·246
1784	58 1205	9 31·8	58 2	283·4	5·60	9·4 12·0	2	·272
1785	59 1286	10 0·4	59 37	218·3	5·56	9·0 10·1	2	·220
1786	59 1365	11 11·8	59 33	336·3	4·35	9·4 9·5	2	·272
...	59 1397	11 36·0	59 29	352·4	30·53	8·6 8·9	2	·329
				352·9	29·67	...	3	·371 M
1787	59 1418	12 2·7	59 14	130·8	5·66	9·5 12·0	2	·329
				132·5	5·00	...	3	·371 M
1788	59 1430	12 9·8	59 26	184·8	2·96	9·9 10·2	2	·329
				182·0	3·37	...	3	·371 M
1789	59 1447	12 27·4	59 26	190·8	1·75	9·5 9·6	2	·424
1790	59 1515	13 26·1	59 36	250·7	4·47	9·5 9·7	2	·424
1791	59 1549	13 57·0	59 10	165·1	1·54	9·5 9·7	1	·408
1792	59 1550	13 58·5	59 21	165·1	3·25	9·1 10·7	1	·408
...	58 1622	16 7·1	58 12	140·8	12·30	7·2 11·5	3	·591
1793	58 1625	16 10·6	58 3	57·4	5·87	8·2 10·5	2	·593
1794	59 1801	17 14·6	59 0	140·2	3·90	9·5 9·6	3	·591
...	55 1934	17 19·7	55 43	63·5	10·81	9·1 11·0	2	·613
1795	...	17 32·7	60 13	151·5	3·27	10·6 11·4	2	·683
...	60 1788	17 51·5	60 31	106·9	12·08	9·4 9·8	2	·694
				109·6	12·12	9·6 10·2	1	·687 M

No.	B.D.	R.A. 1900.	Decl.	P.A.	D.	Mags.	Nts.	Date.	
		h m	° ′	°	″			1919.	
...	+60 1827	18 24·6	+60 25	272·6	45·40	8·2 8·8	1	·682	
				268·8	44·81	8·5 9·0	2	·689	M
1796	60 1880	19 2·0	60 55	102·9	6·39	9·2 13·0	2	·709	
1797	60 2038	19 51·8	60 36	296·3	5·20	9·5 11·0	2	·705	
1798	60 2048	19 53·9	60 33	313·8	5·24	9·5 9·6	3	·708	
1799	58 2048	20 2·9	58 38	153·7	2·08	12·5 12·7	3	·648	BC
				278·0	21·72	A=9·5	2	·642	AB
...	58 2169	20 43·1	58 44	96·7	12·33	9·5 9·6	1	·652	
				95·6	12·22	10·0 10·3	2	·689	M
1800	57 2343	21 28·5	57 27	88·7	3·14	9·0 12·5	2	·767	
1801	56 2667	21 55·7	56 28	106·7	6·51	8·5 13·5	2	·919	
1802	...	22 19·2	57 33	78·2	4·01	10·2 10·3	3	·778	
1803	...	23 35·2	58 13	145·8	2·59	10·5 11·5	4	·856	
1804	57 2823	23 46·9	58 10	80·1	4·75	9·2 12·5	2	·784	
1805	56 3140	23 59·6	56 54	43·2	2·54	9·5 12·0	2	·998	

Notes.

1783, $9^h\ 29^m{\cdot}3$.—In field at P. 221°·3 with B.D. +58°·1199.
1786, $11^h\ 11^m{\cdot}8$.—Also a 10·5 mag. at P. 127°, D. 36″.
1795, $17^h\ 32^m{\cdot}7$.—In low power N. of B.D. +60°·1764.

The following doubles have been detected by Mr. Milburn with the 17¼-inch, and measured with the 24-inch.

No.	B.D.	R.A. 1900.	Decl.	P.A.	D.	Mags.	Nts.	Date.	
		h m	° ′	°	″			1919.	
107	+57 91	0 23·8	+57 27	120·0	5·50	9·5 11·7	2	·883	BC
				184·6	34·28	A=9·5	1	·783	AB
108	...	0 33·1	57 30	66·6	1·42	9·5 10·1	2	·835	
109	...	0 33·4	58 10	65·0	1·62	9·4 9·7	3	·823	
110	57 240	1 12·3	57 51	243·2	2·22	8·7 12·2	3	·987	
111	...	1 32·1	56 20	45·6	1·96	10·1 10·6	5	·218	AB
				228·3	9·88	C=10·2	5	·218	AC
112	...	1 39·2	55 40	41·0	2·10	9·6 9·8	3	·949	
113	56 461	2 8·1	57 7	18·6	5·06	8·6 11·2	3	074	
	56 667	2 30·2	56 27	175·5	8·50	8·7 9·2	2	·059	
114	55 698	2 37·0	56 0	17·3	6·66	8·8 10·1	3	·076	
115	...	3 8·4	57 48	38·6	4·10	9·5 9·8	3	·975	
116	...	3 20·2	56 4	45·5	3·70	10·5 10·6	2	·057	AB
				113·7	13·82	C=10·6	2	·089	AC
117	57 791	4 12·7	57 33	71·8	5·30	8·7 9·9	3	·074	
118	56 924	4 20·5	56 46	164·8	6·12	9·2 10·2	2	·969	

No.	B.D.	R.A. 1900.	Decl.	P.A.	D.	Mags.	Nts.	Date.	
	°	h m	° ′	°	″			1919.	
119	+59 1029	6 44·7	+59 18	306·4	2·56	9·5 9·6	3	·098	
120	58 1012	7 6·5	58 41	273·3	4·00	9·4 9·5	2	·111	
121	...	8 2·5	59 53	5·6	3·56	10·0 10·2	2	·189	
122	56 1271	8 2·8	56 36	224·9	4·01	9·5 12·8	4	·138	
123	55 1291	8 36·5	55 6	26·6	2·04	9·5 9·6	2	·169	
124	57 1207	9 5·2	56 59	107·9	5·49	9·5 9·7	2	·158	
125	55 1360	9 54·8	54 54	144·3	5·33	9·4 11·0	2	·158	
126	55 1367	10 3·3	54 49	252·1	4·29	9·3 9·4	1	·167	
				254·3	4·30	9·5 9·8	1	·159	M
127	57 1264	10 14·6	56 55	227·8	2·41	9·3 9·4	1	·172	
				226·6	2·34	9·5 9·8	1	·172	M
128	57 1279	10 31·1	57 10	39·9	7·12	9·5 11·9	2	·272	
129	54 1392	10 37·8	54 46	136·5	2·62	9·3 11·1	3	·203	
130	...	10 46·8	55 34	70·5	1·34	9·3 10·6	4	·252	
131	57 1371	12 18·3	56 56	148·0	7·45	9·4 11·8	2	·339	
...	56 1596	12 22·4	55 52	231·8	9·77	9·5 10·5	3	·398	
132	57 1470	13 49·6	57 19	354·0	7·04	10·4 10·5	2	·392	
133	56 1695	13 51·9	56 26	240·6	2·89	9·2 10·3	2	·392	
134	57 1516	14 30·1	57 16	47·0	5·12	9·4 12·0	2	·404	
135	55 1813	16 4·2	54 57	98·6	3·66	9·1 9·2	2	·580	
136	56 2102	18 26·0	56 18	243·1	2·05	9·5 13·0	4	·617	
137	56 2137	18 44·0	56 38	166·7	1·89	9·5 9·5	5	·616	
138	...	18 54·3	55 29	64·2	4·58	9·5 12·0	2	·608	
139	60 1876	19 0·4	60 19	127·4	5·85	9·5 11·5	2	·608	
140	60 2114	20 19·5	60 22	118·0	5·97	9·1 12·5	2	·705	
141	...	20 44·3	60 40	293·3	4·56	10·0 12·3	4	·785	
142	60 2253	21 27·8	60 53	325·0	3·95	9·5 13·0	2	·724	
143	56 2648	21 45·3	56 54	349·5	1·93	9·5 9·6	2	·668	
144	...	22 18·4	55 25	294·8	2·27	9·5 12·5	4	·782	
145	55 2780	22 32·5	55 24	9·5	2·35	9·0 11·2	3	·756	
146	56 3001	23 20·2	56 17	143·0	1·81	9·0 11·5	3	·862	
147	55 3014	23 40·8	55 32	312·6	2·23	9·3 11·2	3	·782	
148	55 3017	23 42·2	55 35	259·2	4·43	9·5 9·7	2	·767	
149	57 2810	23 43·3	57 20	268·7	2·80	9·5 11·0	2	·983	AB
				170·2	11·45	C=9·6	3	·987	AC

Notes.

139, $19^h\ 0^m\cdot4$.—Also a faint star at P. 272°, D. 37″·1.

144, $22^h\ 18^m\cdot4$.—In field with B.D. +55° 2734 at P. 303°·2.

149, $23^h\ 43^m\cdot3$.—A distant 12 mag. at P. 244°·5.

Missing Stars.

		h m s	° ′		
1	+57°84	0 19 19·5	+57 11·7	Mag. 9·5	1855·0
2	58·1131	8 22 0·5	58 3·3	,, 9·5	,,
3	59·1210	8 50 19·8	59 12·8	,, 9·5	,,
4	58·1165	8 50 21·0	58 40·5	,, 9·1	,,
5	55·1463	11 17 49·6	55 10·4	,, 9·4	,,
6	57·2361	21 35 19·7	57 25·1	,, 9·5	,,

Notes.

(1) Decl. should be 4′ n. of B.D. place.
(2) 1919 March 17, 11·5 mag., so April 12.
(3) 1919 March 5, April 8, no star here. No star in H.M.S.
(4) 1917 March 17, April 8, only 9·7 mag. A.G. 9·5. H.M.S. about 9·6.
(5) Milburn, 1919 March 3, not seen. H.M.S. no star.
(6) 1919 Oct. 2. 12 mag. only.